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THE MIXING QUESTION, VIEWED THEORETICALLY AND PRACTICALLY, TOGETHER WITH A CONSIDERATION OF INTERNAL WAVES

REPORTS OF THE PROCEEDINGS OF A SPECIAL MEETING HELD
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REPORT

On Internal Waves[1]).

By

V. Walfrid Ekman.

Chairman and Gentlemen.

I am afraid that you ought not to have charged me with the report on internal waves. For I have not been working at that subject at all, except at a quite special problem which has very little connection indeed with oceanography. What I can give you is merely a survey of the work of other scientists, sometimes with a criticism of a chiefly negative character; and I regret that I cannot offer any essential positive contribution to the subject myself.

Vertical movements of internal boundary-surfaces in the sea were, as far as I know, observed for the first time by NANSEN on his North Polar Expedition (1902, p. 346 seq.). Since then similar observations have been repeatedly made by himself (1913) and by him and HELLAND-HANSEN, who allot to the matter a separate chapter in their large work: "The Norwegian Sea" (1909). The authors point out the necessity, on account of these oscillations, of using a close net of hydrographic stations; since there is a risk of otherwise hazarding the trustworthiness of the results.

Professor OTTO PETTERSSON (1907) discovered vertical internal movements in the Danish sounds, and these movements seem to have shown a tidal periodicity. Prof. PETTERSSON followed up his study of the phenomenon with admirable energy and consistency by arranging for many years special daily observations in a Swedish fjord, where similar internal motions were found to exist.

Important observations on the matter have since then been made by various expeditions.

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[1]) Parts of this paper are worked out somewhat more fully than they were in my lecture. This is particularly the case with the theoretical part. Bibliographical references and a few more figures have also been added.

Theoretical.

Laws of Propagation.

Before critically examining this material it is necessary to make a few remarks on the dynamics of internal waves, but I will try to be as brief as possible on this point. Further information may be obtained from the excellent work on Hydrodynamics by HORACE LAMB or other text-books.

We may confine ourselves to the case of waves with straight, parallel wave-fronts moving in the boundary between two homogeneous water-layers: namely one homogeneous "deep-water layer" covered by a comparatively thin "top layer" of lighter water.

The characteristic features of the wave-motion is indicated in Fig. 1, where the long, straight arrow indicates the direction of the velocity of propagation v of the waves,

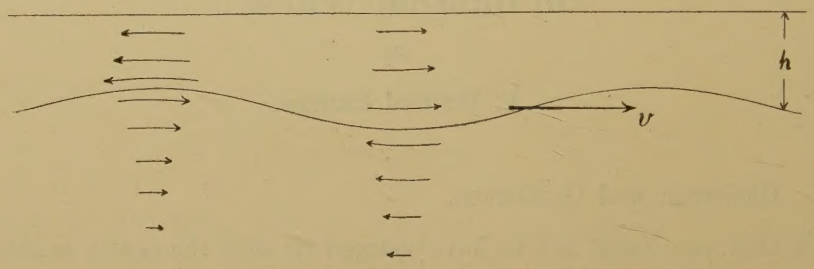

Fig. 1.

whereas the shorter arrows are intended to give an approximate idea of the motion of the water-particles themselves in the different parts of the wave. It is important to note that the velocities of the water-particles have opposite directions in the deep-water layer and in the top layer, when the horizontal components of velocity within a vertical column of water are considered; and the same is the case with the accelerations.

The effect of the motion on the shape of the sea-surface is imperceptible and cannot be represented in the figure. The wave-energy is small, being in proportion to the difference in density $\Delta\varrho$ between the water-layers, as is shown in the cases of progressive waves and standing waves by equations (1, a) and (1, b) respectively.

$$E_p = \frac{1}{2} g a^2 \Delta\varrho; \qquad (1, a)$$

$$E_s = \frac{1}{4} g a^2 \Delta\varrho; \qquad (1, b)$$

$$v = \sqrt{gh\frac{\Delta\varrho}{\varrho}}. \qquad (2)$$

E_p = energy of progressive waves per unit area of sea-surface,
E_s = energy of standing waves per unit area of sea-surface,

v = velocity of propagation of long progressive waves,
g = gravity,
a = wave amplitude in the common boundary,
h = depth of top layer,
ϱ = density of top layer,
$\varrho + \Delta\varrho$ = density of deep-water layer.

The velocity of propagation is also small but varies as the square root of the difference in density. Conditions are comparatively simple in the cases of "short waves" (wave-length not exceeding the depth of the top layer) and of "long waves" (wave-length more than say 10 or 20 times the depth of the top layer). In all cases of oceanographical interest the internal waves are decidedly of the "long-wave" type. The velocity of propagation of such waves is given approximately by equation (2); and the wave-amplitudes in the sea-surface and in the common boundary are approximately in such a ratio to one another that the deep-water is in hydrostatic equilibrium.

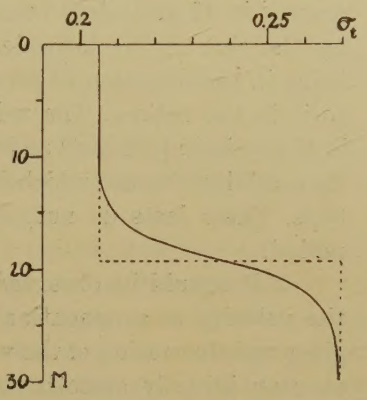

Fig. 2.

Actually the water-layers in the sea are not homogeneous, but there is a gradual transition of density from the heavier deep-water to the lighter surface-water. I shall not trouble you with elaborate mathematical theories that have been developed with regard to this circumstance. I only mention that on certain special conditions as to the vertical distribution of density Prof. ZEILON (1912, 1915) has found a simple way of applying the proper correction to the velocity of propagation of the waves. For example in the case represented by the full-drawn line in Fig. 2 the velocity of the internal waves would be about 10 % less than what it would be in the case of two homogeneous layers, separated by a surface of discontinuity, as represented by the dotted line.

The length of the waves has an important consequence which ought to be mentioned. "Short waves", as for instance the waves produced on the sea-surface by the wind or round a stone thrown into the water, have the well-known property that each individual wave continually leaves part of its energy behind in the form of a new wave. In this way a limited train of waves gradually spreads over an ever increasing area, while the wave-amplitude correspondingly decreases; and at the same time the wave-lengths and amplitudes within a limited area become more and more uniform, if they were not so from the beginning. Internal waves will behave more or less in the same way if their wave-length is less than say, 10 times, the depth of the top layer. Waves of the long-wave type, with which we are concerned, do not have this property, but each single wave proceeds with unaltered shape and height except for the abatement due to friction. Of course a ring-shaped wave, initially produced by a disturbance at the centre of the ring, decreases in amplitude, according as the ring widens, but otherwise without any alteration of the wave-profile.

Influence of the Earth's Rotation.

As yet we have not taken the earth's rotation into consideration. Waves of sufficiently long periods will, however, be substantially influenced by the Coriolean force, the water-particles undergoing during their motion a continual deflection *cum sole*, that is to the right in the northern hemisphere. With ordinary surface-waves in view H. U. SVERDRUP has drawn important consequences from this circumstance, consequences which can be shown to hold good in their essentials for internal waves as well. In a remarkable paper on tides (1926) he has, among other theoretical investigations, made some calculations on tidal waves in a wide ocean on the rotating earth, assuming that the wave-crests are straight and that they travel in a direction normal to the crest-lines. The character of the free motion was found to depend essentially on its period s as expressed in 12 pendulum hours as a unit. (A pendulum hour means a sidereal hour divided by the sine of the latitude.) Each water-particle describes an ellipse, the major axis being in the direction of propagation of the waves and the minor axis being proportional to it in the ratio s. The velocity of propagation v is increased by the earth's rotation in the ratio $1 : \sqrt{(1-s^2)}$ and tends to infinity when the wave-period approximates towards 12 pendulum hours, which is the greatest possible period of free waves of the assumed type. These facts do not, of course, eliminate the possibility of forced waves of any period.

It should be observed that the velocity of propagation v here discussed is merely the velocity of propagation of an endless train of simple-harmonic waves, and that it gives no information of the velocity with which a local disturbance of equilibrium spreads over an initially smooth water-surface. To get an idea of this latter quantity we fall back upon the so-called group-velocity which represents the rate at which the energy of a train of simple-harmonic waves is transmitted in the direction of propagation of the waves. This group-velocity U may be calculated from the equation

$$U = v - l\frac{dv}{dl}, \tag{3}$$

where v denotes the velocity of propagation of the waves, regarded as a function of the wave-length l.

In the case of "short waves", the velocity of which increases with the wave-length, equation (3) shows that the group-velocity is smaller than the velocity of propagation of the individual waves, and this is the reason why such waves have the property of spreading, leaving continually new waves behind them. "Long waves" have a velocity of propagation which is independent of the wave-length (*i. e.* $\frac{dv}{dl} = 0$), so that $U = v$. When, however, the wave-length and wave-period are further increased, so that the motion is markedly influenced by the earth's rotation, the velocity of propagation of the waves begins again increasing with increasing wave-length, and therefore the group-velocity once more falls short of the velocity of propagation. As a matter of fact: though v is increased, U is found to be diminished on account of the earth's

rotation, namely in the ratio $\sqrt{(1-s^2)}$, and it vanishes altogether in the limiting case $s = 1$, when the velocity of propagation of the individual waves becomes infinite.

It is easily proved that quite similar results will hold for internal waves, at any rate if the wave-length and the depth of the deep-water layer are both great as compared to the depth of the top layer. The water-particles move in ellipses, the axes of which are in the ratio s, and the velocity of propagation of the waves is found to be

$$v = \sqrt{gh\frac{\Delta\varrho}{\varrho}} \cdot \sqrt{\frac{1}{1-s^2}}, \tag{4}$$

whereas the rate of transmission of wave-energy (the group-velocity) is

$$U = \sqrt{gh\frac{\Delta\varrho}{\varrho}} \cdot \sqrt{1-s^2}. \tag{5}$$

The greatest velocity at which a local disturbance of the internal boundary can spread is therefore equal to the velocity of propagation which would take place on a non-rotating earth, and which is given by equation (2).

The elliptical orbits of the water-particles pass into circles, and the velocity of propagation becomes infinite, when the wave-period is 12 pendulum hours, which is its maximum value. Thus for instance free internal waves of a lunar semidiurnal period would not be possible at higher latitudes than 75°, and waves of a lunar diurnal period not beyond 29°. Waves of a fortnightly period would only be possible within the latitudes of 2°. As already mentioned, these statements have no bearing upon forced waves. Furthermore it should be emphasized that they do not interfere with the possibility of long-period waves in channels or even along a coast, where the deflection of the motion from its main direction must obviously give rise to variable and alternating inclinations of the internal boundary transverse to the direction of the wave-motion. For the pressure-gradients resultant upon these transverse inclinations will check the deflective Coriolean force, so that in a sufficiently narrow channel the waves will be propagated nearly uninfluenced by the earth's rotation. But in a comparatively wide channel there will not be sufficient time during a wave-period for the transverse inclinations to be formed, and in that case the modifying influence of the earth's rotation will be actually effective. Obviously this will be the case if the width of the channel is of the order of magnitude of a wave-length or more, and remembering what has been said about the spreading of a local disturbance of boundary-level it is clear that in this connection the wave-length should be calculated from the given wave-period by means of the narrow-channel formula, equation (2).

It might be objected against the theoretical impossibility of long-period waves that actually diurnal tidal components occur at much higher latitudes than 30°. It should be remembered, however, that diurnal tidal waves in the deep sea have such great wave-lengths (according to the narrow-channel theory) that with these wave-lengths as the unit of distance a considerable part of the ocean ought to be regarded as adjacent to

the coast. Conditions will be quite different in the case of internal waves, which never attain similar wave-lengths. As an example we may consider the conditions at one of the anchor stations (Nr. 288) of the "Meteor"-Expedition to be mentioned presently. The top layer was 100 metres deep, and the difference in density between the deep-water and the top layer was uncommonly great, namely 0.003. But nevertheless the velocity of the waves would be less than 2 metres per second according to the narrow-channel formula, and the wave-lengths of internal waves would be less than one hundredth of the wave-length of surface-waves with the same period. For diurnal narrow-channel waves it would be about 180 kilometres only.

Abatement by Friction.

Free internal waves must abate owing to internal friction and eddy-viscosity. Since we know too little about the magnitude of the latter, it would not be possible at present to answer accurately or even approximately the important question as to the rate of decay. G. I. Taylor (1919) has successfully attacked an analogous tidal problem by means of the notion of skin-friction at the sea-bottom. This notion of skin-friction, however, is not applicable to the present problem, where friction is supposed to act chiefly in the neighbourhood of an internal boundary; and it would not permit of taking into consideration the more or less stable stratification of the water near the boundary.

I have made an attempt to calculate the rate of decay from the assumption of a constant coefficient of virtual viscosity, though of course even this method is only acceptable for want of a better one. The calculation, of which the main points only are reproduced here, is merely intended to give some hints as to the probable order of magnitude of the frictional effect; and the earth's rotation is not taken into account.

To begin with, consider the case of standing, long waves. If for a first approximation we assume the same type of motion as would take place in the absence of friction, the upper water-layer or "top layer" would oscillate nearly horizontally after a simple-harmonic law, the velocity u of a water-particle being given, in the same notation as before, by the formula

$$u = \frac{al}{hT} \cdot \cos \frac{2\pi x}{l} \cdot \cos \frac{2\pi t}{T}, \qquad (6)$$

where t means the time and x means the horizontal coordinate of the particle, reckoned in the direction of propagation of the waves. The corresponding motion of the lower water-layer is negligible, if its depth and the wave-length are both great as compared to the depth of the top layer. Under these conditions the disturbing effect of viscosity would be equivalent to the superposition of a system of "viscosity waves" issuing from the internal boundary on account of the boundary-shear (equation 6) and travelling vertically in both directions through the respective water-layers with a wave-length

$$\lambda = 2\sqrt{\pi \mu T},$$

where μ denotes the virtual, kinematic coefficient of viscosity. The amplitude of these viscosity-waves diminishes rapidly with increasing distance from the boundary *viz.* in

the ratio $e^{-27} = 1 : 535$ when the distance is increased by λ. The rate of dissipation of energy by friction, per unit area of the boundary, is calculated to be

$$\frac{a^2 l^2 \varrho}{8 h^2 T^3} \sqrt{\pi \mu T}, \tag{7}$$

if both water-layers are considered, and if λ is small as compared to h. Multiplying (7) by T and considering equations (1, b) and (2) as well as the identical relationship $Tv = l$ it is found that the energy is, during each wave-period, diminished by

$\frac{1}{2h} \sqrt{\pi \mu T}$ times its actual amount, whereas the relative rate of decay of the wave-amplitude is half as great.

The same result is found to hold for progressive waves, and it can be expressed — for standing as well as for progressive waves — by stating that their logarithmic decrement is approximately

$$\delta = 0.06 N, \tag{8}$$

where

$$N = \frac{1}{h} \sqrt{\mu T},$$

μ = virtual coefficient of viscosity in C.G.S.-units,
T = the wave-period in hours,
h = the depth of the top layer in metres.

In the same notation the wave-amplitude is, during each wave-period, diminished by about

$$n = \frac{1}{4} N \text{ times its actual amount.} \tag{9}$$

As already mentioned, this calculation is made on the assumption that λ is small as compared to h, but the result can be regarded as approximately or at any rate roughly correct as long as λ does not exceed h. In the (rather arbitrarily chosen) limiting case $\lambda = h$ we have $N = \frac{1}{2}$ and $n = \frac{1}{8}$.

The most serious defect of this theory is that the virtual coefficient of (eddy-) viscosity was assumed to be equal in all levels, whereas in reality it is likely to be greatest within the comparatively homogeneous water-layers and much smaller in the neighbourhood of the quasi-boundary, where the stratification is a decidedly stable one. It is then easily understood that some mean value of μ must be used if the decrement should be correctly calculated, but also that predominant weight must be attached to the values of μ in the vicinity of the "boundary", since the velocity of shear is greatest there. On the other hand, in the common case of a gradual transition from the density of the deep-water to that of the top layer equation (8) or (9) may under certain conditions give too large a value of the decrement, since the dissipation of energy must be diminished on

disappearance of the discontinuous boundary-shear. As a rule the said equation probably determines an upper limit of the decrement resultant from a given value of μ.

Unfortunately a quantitative application of equation (8) is not possible at present, since too little is known about the absolute magnitude of μ. From various observations it is found sometimes to reach the order of magnitude of 100 or 1000 C.G.S.-units — though of course not in water with markedly stable stratification — and under other circumstances it may be as small as perhaps one or two units. Even if it never comes down to its theoretical minimum of about 0.02 — which would mean that eddy-viscosity were failing altogether — it can nevertheless vary within very wide limits, and we have no means to determine it but by observation of the motion itself.

Experience.

Internal Seiches in Lakes and Fjords.

Turning now to the observations, I wish first to remind you of the standing internal waves in lakes, which have been described by Wedderburn (1911 and other papers) and by Exner (1928 and other papers) under the name of temperature-seiche. It is of interest to note that in this case the observed periods of oscillation — as a rule of the order of magnitude of a day — were in agreement with the velocity of propagation of internal waves as theoretically calculated. It is furthermore of interest to note that according to observations the initial disturbance of equilibrium seems to have been produced by the wind sweeping the warmer and lighter top-layer of water from one end of the lake to the other. After each disturbance the subsequent oscillations abated during the lapse of a few wave-periods, which is also in agreement with a theoretical decrement calculated from a reasonable value of the eddy-viscosity. On the whole it seems to me that there is at present no fundamental question with regard to internal seiches in lakes which is unsolved in its essentials, unless perhaps the influence of the earth's rotation.

The most comprehensive series of observations in the sea are undoubtedly those arranged by Prof. Otto Pettersson in the Gullmar Fjord (see papers by O. Pettersson 1909 and other papers and by N. Zeilon 1912). They are all made at one place only — at the Swedish hydrographical Station at Bornö, some 10 kilometres from the upper end of the 30 kilometres deep fjord. But they have been pursued for several years, and daily observations of temperature and salinity at various levels down to 40 metres have been taken; and furthermore during part of the time the variations of level of one particular isodensic surface, indicative of the internal motions, have been automatically and continuously recorded.

The motion shows a rather confused character, as is seen in Fig. 3, representing the depths of the isohalines for 30 $^0/_{00}$ and 33 $^0/_{00}$ plotted against time from Jan. 31st to June 16th 1909 and between the same dates in 1911[1]). At intervals it assumes the appearance of comparatively regular waves of various periods as *e. g.* 14 days, 7 days,

[1]) The figure is a copy, made by hand and extracted from a larger and more comprehensive diagram in "Svenska Hydrogr.-Biologiska kommissionens skrifter" vol. V.

and so on; but they never last for more than a very few periods each time, and they may easily be interpreted as irregular motions, which accidentally assume the appearance of seeming regularity. Exception should be made for two modes of oscillations, for which Prof. ZEILON (1912) has found the necessary dynamical explanation, and which sometimes last for a comparatively large number of periods. These modes are likely to be really regular waves. (See, however, the foot-note.)

The period of the first mode — 2 or 3 days — agrees with a theoretically calculated period of standing, internal waves in the fjord. Such waves, which are therefore likely to represent an ordinary boundary-seiche[1]), are seen in the 33 $^0/_{00}$ isohaline from May 20th to June 14th 1909.

The second mode, the so-called "Secondary boundary-seiche", has been made the subject of a close mathematical and experimental study by Prof. ZEILON (1911) with the following result: If the bulk of water (deep-water and top layer) is performing an alternating motion like *e. g.* a tidal- or seiche-current, and if it has to pass a bottom ridge or a hump or the like, the disturbing effect of the latter will reach the top layer and will give rise in the common boundary to an internal wave-motion, emerging from the place of disturbance and having the same period as the original alternating current. The investigation was limited to the case of two-dimensional motion through a channel,

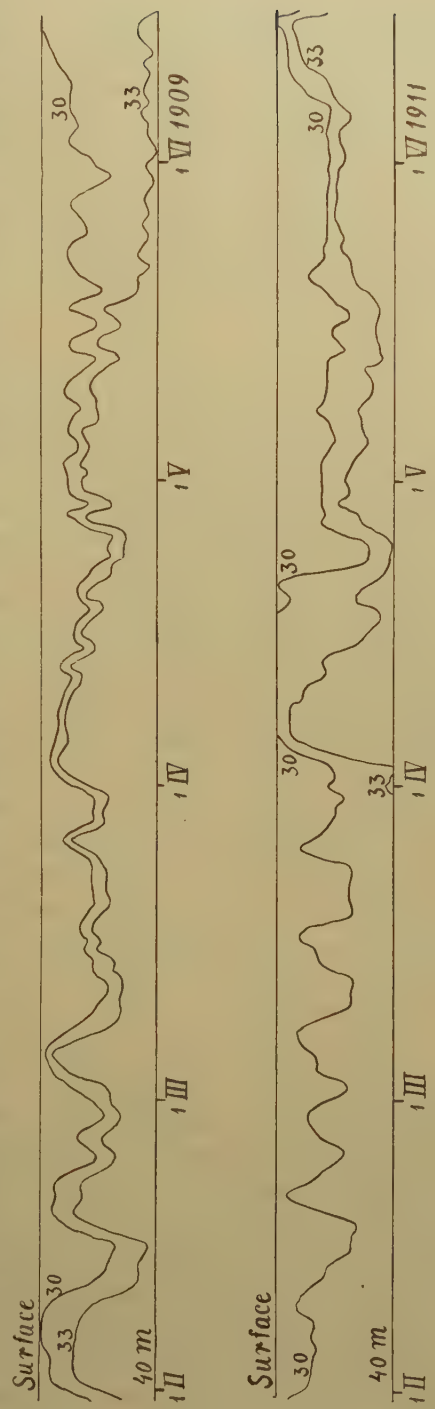

Fig. 3.

[1]) On revising this report for publication I am now less inclined to accept this interpretation of the said apparent waves. As real free seiche-waves they would not last for so long a time. As a matter of fact their amplitude would, according to equation (8) or (9), diminish in the ratio 1 : 2 in less than 3 wave-periods, even if the virtual coefficient of viscosity were 4 C.G.S.-units only. On the whole the observed "waves" do not show any gradual decay, but last for a time with a somewhat varying amplitude and then disappear. On the other hand it is not at all necessary to regard their periodicity as real, since the presumed period is only 2 or 3 times the interval between two successive observations. In this respect I may refer to similar remarks on pp. 28—29.

on the bottom of which there is a transverse ridge, and in this case it was shown that the internal waves may attain much greater amplitudes than that of the surface-waves connected with the original current. If the channel is closed at one end, standing waves may be generated inside the ridge. In the Gullmar Fjord Prof. Zeilon found at Bornö a surface-seiche with a period of about 110 minutes and a double amplitude of 14 cm.; whereas the amplitude of the corresponding "secondary boundary-seiche" at the same place was found to be 3 times as great or somewhat more. The latter became visible in the automatical continuous records, but it cannot be seen in Fig. 3, which represents the results of daily observations only.

The Cosmic Theory.

The long-period oscillations are of still greater interest in so far as they more thoroughly affect the physical and biological conditions. They have also a much greater amplitude, sometimes as much as 20 or 30 metres in height. Prof. Otto Pettersson has found a coincidence in some cases between the crests of these waves on the one hand and lunar epochs (maxima of the vertical component of the tide-generating force) on the other. This is the case for instance with the 3 large wave-crests which appear in February and the beginning of March 1909 (see Fig. 3).

Prof. Pettersson has pursued this observation in a large number of papers (1909, 1914, 1921, 1930, and others), and he has propounded the theory that these long-period oscillations are as a matter of fact due to the small monthly variations of the tide-generating forces. I confess that I don't see how to bring his theory into agreement with dynamical principles, and I will come back to that point presently. But I wish to say that — as always in my negative criticism — I will only refer to general principles and avoid going into much detail, leaving that for the discussion. Not only will the continuity of the survey, which I wish to give you, profit by this arrangement, but also the coherency of the subsequent debate on special problems. I wish to say this beforehand, because you might otherwise get the impression that I have not given Professor Pettersson's theory the attention which its important intentions and the authority of its originator demand.

Since the theoretical support of the presumed correlation with lunar epochs is at any rate open to doubt, the observed coincidences must of course be so much the more numerous and unmistakable if they are to be altogether convincing. As a matter of fact they reappear nicely sometimes, but at other times they are not by far so good or they fail altogether. Professor Pettersson has made an attempt to verify the correlation by means of harmonic analysis but, according to his own statement, without success. He then declared — if I have understood him correctly — that it would be a mistake to expect permanent periodicity of phenomena like these, and that therefore the harmonic analysis must necessarily fail when carried out for too long a space of time; and for analogy he referred to the sun-spots, and to the train of waves in a sea-swell. Of course this argument may be true in the case of a phenomenon like the sun-spots, the cause of which is unknown, and it obviously is true when, as in the case of

a sea-swell, the periodicity is only approximate and produced by the gradual smoothing and sifting of initially irregular oscillations. But the present problem is essentially different. It is maintained that the oscillations are made up of components produced by periodic forces with various known periods such as the synodic month, the anomalistic month, and so on. If this hypothesis is true — and this is the very question to be settled — the periods of a force and of the corresponding component produced by it must of course be exactly the same. It should be possible therefore to verify the hypothesis by means of harmonic analysis, carried out for each period separately, and the result arrived at ought to be better, according as the analysis is extended over a longer space of time. When this demand is not answered, I think we must look for another explanation of the long-period internal movements.

Generation of Long-Period Oscillations by Winds.

For this purpose it is natural to think of the varying winds sweeping the surface-water from one place to another and thus causing the deep-water to be raised at the former place and pressed down at the latter[1]) as in the case of temperature-seiche in lakes. Professor Hans Pettersson (1920) examined the possible correlation between the wind's component in the direction of the fjord and the level of a particular isohaline representative of the internal condition of the water-layers. The investigation was based on three days' average values of the correlated quantities and was carried out for each month separately for nearly 3 years 1909—1911. An abstract of the result is seen in the following table.

Number of months	$\frac{r}{\varepsilon}$	r_m
3	negative	— 0.37
9	0—3	0.15
5	3—6	0.42
11	6—9	0.58
2	9—12	0.67
5	12—26	0.79

In 32 out of 35 months there was found to be a positive correlation, and in the case of 18 months the quotient between the coefficient of correlation r and the probable error ε exceeded 6, which is generally considered sufficient to "prove" the existence of a relationship between the correlated quantities. For each group of months the average coefficient of correlation r_m is given in the 3d column, but the real correlation between wind and internal motion was probably even somewhat more decided than would appear from these numbers, since for various technical reasons the quantities to be correlated could not be chosen in the most favourable manner. In any case I think it must be regarded

[1]) This effect of the winds has been illustrated through a beautiful laboratory-experiment by J. W. Sandström (1908).

as a fact, established by the investigation under review, that the long-period internal movements in the fjord are in a large degree dependent on the winds, even if the connection is not always clearly established and is at times concealed by some other unknown dependence.

Fig. 4, copied from Prof. HANS PETTERSSON's paper, illustrates the connection between the wind and the vertical displacement of the water-layers during 3 months when this connection was comparatively close. (These months are among those in the 4th line of the table, $\frac{r}{\varepsilon}$ lying between 6 and 8; $r_m = 0.57$.) The uppermost curve represents

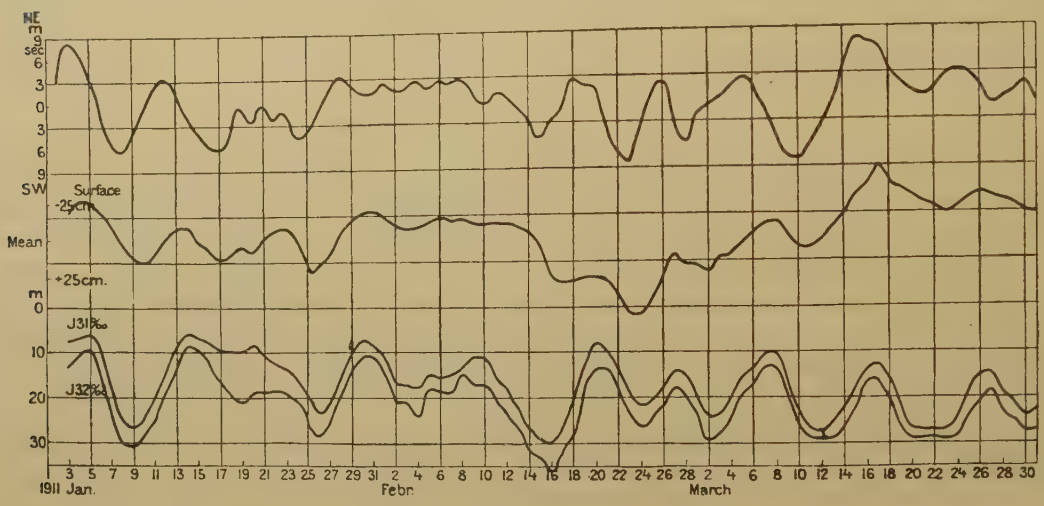

Fig. 4.

the wind's component in the main direction of the fjord as observed off the coast. The next curve gives the variations of surface-level inverted, but we are not concerned in this curve for the present. The two lower curves represent the depth below the sea-surface of two representative isohalines (for 31 and 32 per mille). These latter curves are smoothed out by the formation of average values of the ordinates for three days. A comparison between them and the upper curve makes it quite clear, I think — and similar diagrams for other months give the same result — that the long-period motions in question cannot be regarded in any considerable degree as free waves, started by some initial disturbances. On the contrary they are almost entirely forced oscillations, immediately due to the actual wind (or rather to the winds during the previous one or two days) and to other agencies which cannot be specified for the present. This is in agreement with hydrodynamical principles; for calculation shows that internal waves with such long periods in water-layers of such little thickness must be expected to abate very quickly owing to eddy-viscosity. With a period of 14 days, a 20 m. deep top layer, and a virtual coefficient of viscosity equal to 4 C.G.S.-units only, equation (8) gives a decrement 0.11, which means that the amplitude would be diminished in the ratio 1 : 2 in less than one period and a half.

Fig. 5, which is borrowed from a subsequent paper by Hans Pettersson (1923), shows that the long-period internal motions in the Gullmar fjord are no local phenomenon there, but that they appear simultaneously along a considerable portion of the West coast of Sweden. The ordinates of the curves, reckoned from their respective zero-lines, represent variations of the hydrostatic pressure of, say, the uppermost 25 metres of water, and indirectly they represent therefore in a way the vertical movements of internal boundary-surfaces. The distance between two horizontal zero-lines corresponds to a vertical displacement of about 15 metres of a representative boundary-surface. The three

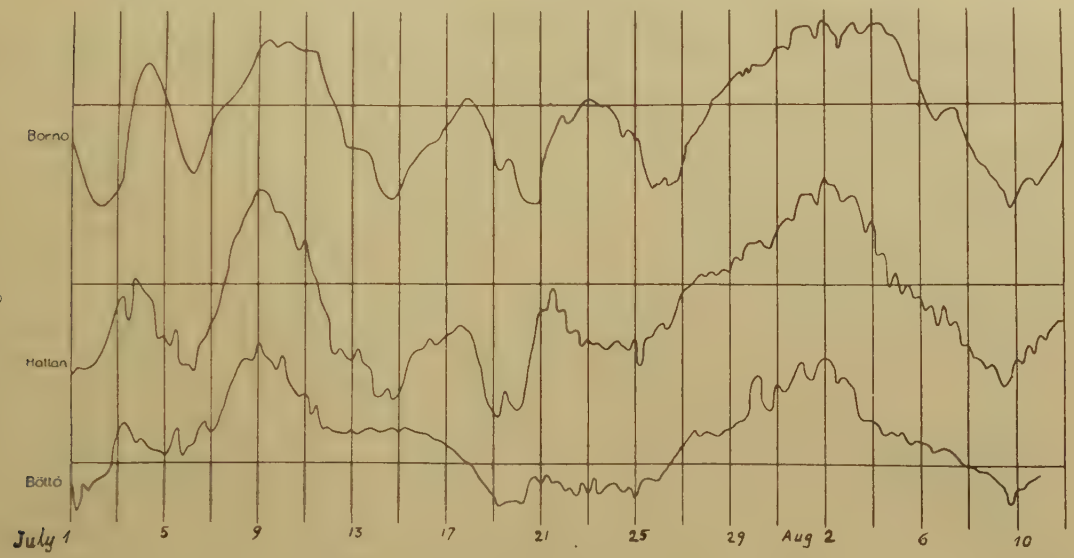

Fig. 5.

curves represent in this manner the simultaneous internal movements during 6 weeks in the summer 1918 at Bornö and at two places: Hättan and Böttö, some 25 and 40 miles farther south along the coast. The parallelism between the three curves is obvious. The lowest curve, representing conditions at Böttö, is in a way truncated in its lower parts, because at Böttö the pressure-gauge could not be applied at a sufficient depth and therefore registered internal movements within the uppermost 19 metres of water only. Some years earlier Hans Pettersson (1916) made an analogous investigation over a larger area but with less perfect technical apparatus. If the necessary means could be raised, I think it would be a most important and valuable task to repeat the investigation on a still larger scale. It might be possible in this way to get an idea of the power of the wind to displace the surface-layers over long distances and of the faculty of these surface-layers to behave as an oscillating system. But it would probably be necessary for that purpose to extend the investigation over rather a long time and over a considerable number of stations within the Skagerack and the Kattegat.

Tidal Motions in the Open Ocean.

Turning to investigations in the open ocean the most remarkable fact is the presence of internal waves with tidal periods. Fig. 6 represents some results of Danish

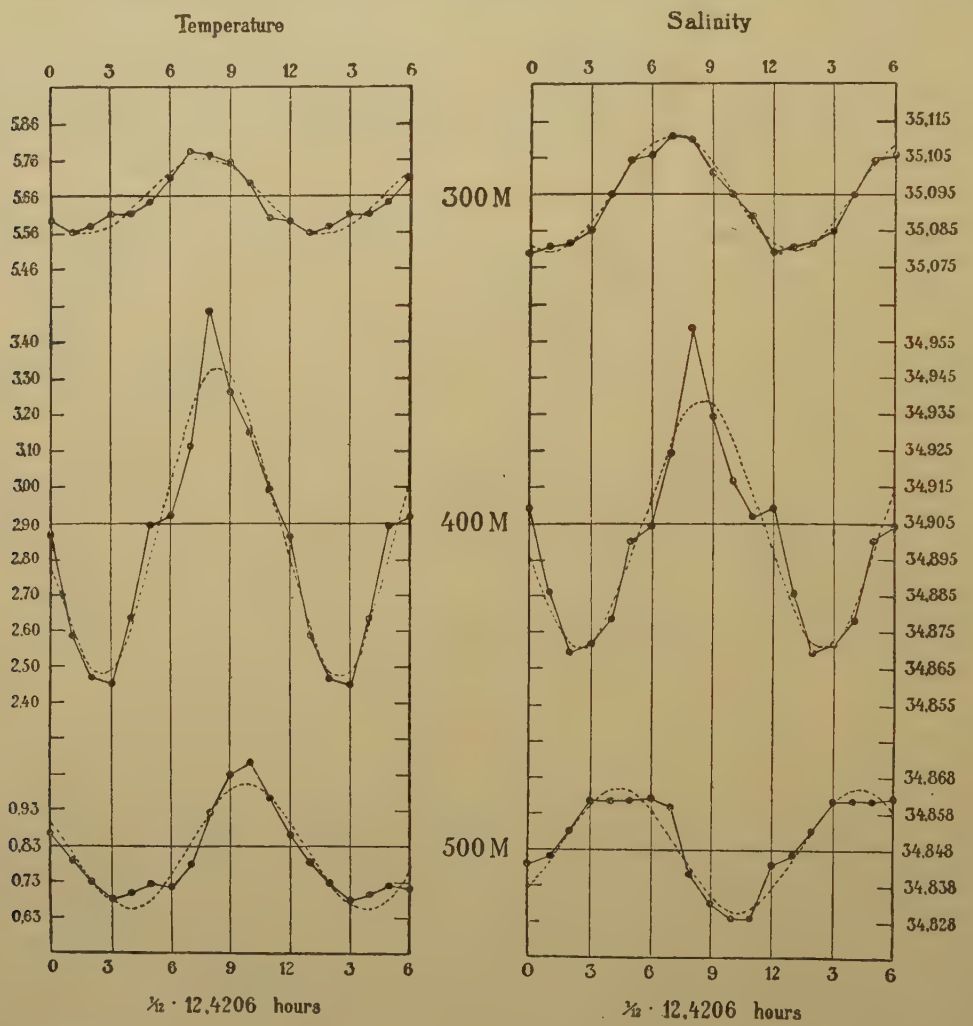

Fig. 6.

observations which were continued for 67 hours in the vicinity of an anchored buoy in the western side of the Faeroe-Shetland Channel. The temperature was in this place decreasing from the surface and downwards, whereas the salinity was decreasing down to 400 metres but increasing below 500 metres. The greatest differences in temperature and salinity were between 300 and 400 metres. Temperature and salinity at depths of 300, 400, and 500 metres are plotted against lunar hours, each dot indicating the average

of the values observed at the same lunar hour, 1 to 12, irrespective of the day. I shall not dwell on other interesting details but simply remark that the reality of semidiurnal oscillations is quite obvious here. There were also found marked indications of diurnal tidal oscillations, though they are perhaps less certain owing to their comparatively small amplitude and to the comparatively small number of wave-periods observed. Of course, since they take place several hundred metres below the sea-surface, they cannot be due to daily meteorological variations.

Scottish and Norwegian investigations, which were performed in the neighbourhood simultaneously with the Danish ones, gave similar interesting results (B. Helland-Hansen 1930).

Semidiurnal oscillations were equally obvious at the anchor station 288 of the German Atlantic Expedition with the "Meteor". The observations at this station have been examined in some detail in a preliminary report by Professor A. Defant (1927), from which Figs. 7—9 are borrowed[1]). The station was situated N. of Brazil in water 4350 metres deep on the south-western side of the Mid-Atlantic Ridge (12°38′ N, 47°36′ W). As is seen from Fig. 7 an unusually pronounced discontinuity-layer was found here at about 80 or 100 metres below the sea-surface with almost homogeneous water in the top layer and an increase in specific gravity of 3 units in the third decimal place between 75 and 200 metres. Nearly half of this increase occurs in the interval between 75 and 100 metres. For 38 hours temperatures and salinities were observed every hour at several depths down to 250 metres. The observed temperatures appear from Fig. 8. Owing to the homogeneity of the top layer the internal waves only occasionally influenced the temperature in 75 metres, and in 50 metres not at all. In the diagram, Fig. 9, the average temperature between 100 and 200 metres is plotted against time, the short-period variations being eliminated by taking the averages of three successive readings — from 1 hour before to 1 hour after the plotted time. I leave other details and only note the obvious semidiurnal period of the oscillations. Perhaps I should note as well the indications of oscillations with a period of about a quarter of a day.

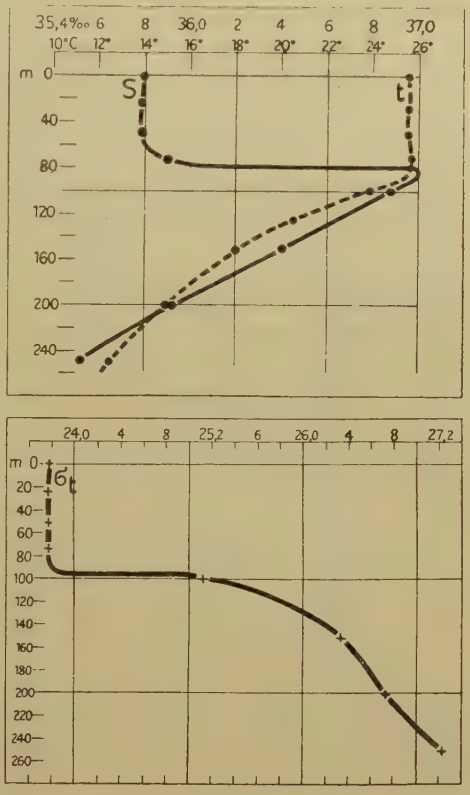

Fig. 7.

Some observations of currents, which Professor Helland-Hansen and the reporter made in the Atlantic last summer, also indicate the existence of tidal internal oscilla-

[1]) See also A. Defant (1929).

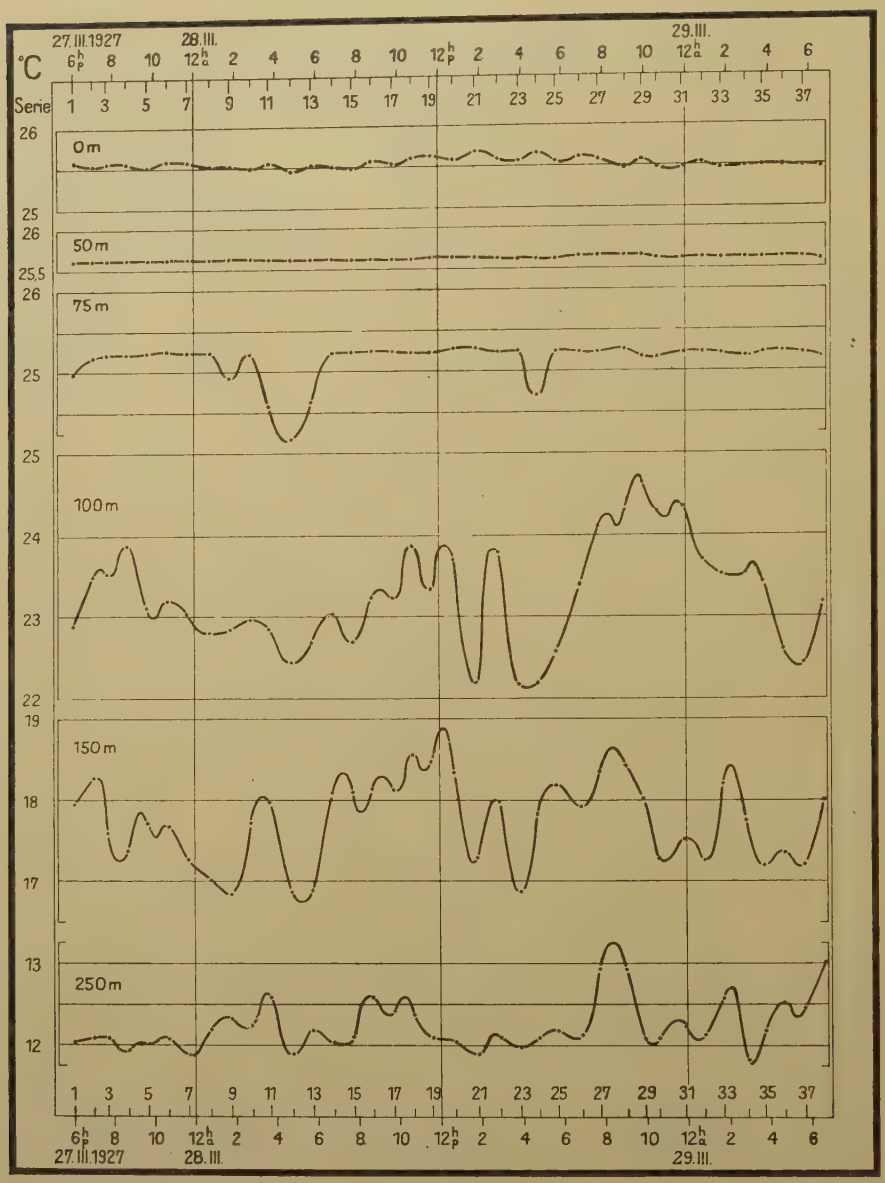

Fig. 8.

tions[1]). We anchored at different places between Portugal and Morocco and the Canaries, and at each place we measured currents continuously for several days, so that the motion in various levels could be subjected to harmonic analysis. The material is not worked up definitively yet, and the curves in Figs. 10 and 11 are to be regarded as preliminary

[1]) Similar observations, though less conclusive, were made already 25 years ago by B. Helland-Hansen (1907, p. 17). See also Helland-Hansen and Nansen (1926, p. 21).

only. They represent in the form of hodographs or so-called "central diagrams" the diurnal and semidiurnal tidal components of the current at various levels: 100, 300, 600, and 1000 metres below the sea-surface[1]). These hodographs are ellipses, but only one half of each ellipse has been drawn, namely the northern (upper) half in the case of the semidiurnal component and the southern (lower) half in the case of the diurnal component. A radius, drawn from the centre of the diagram to a point on the ellipse, represents the

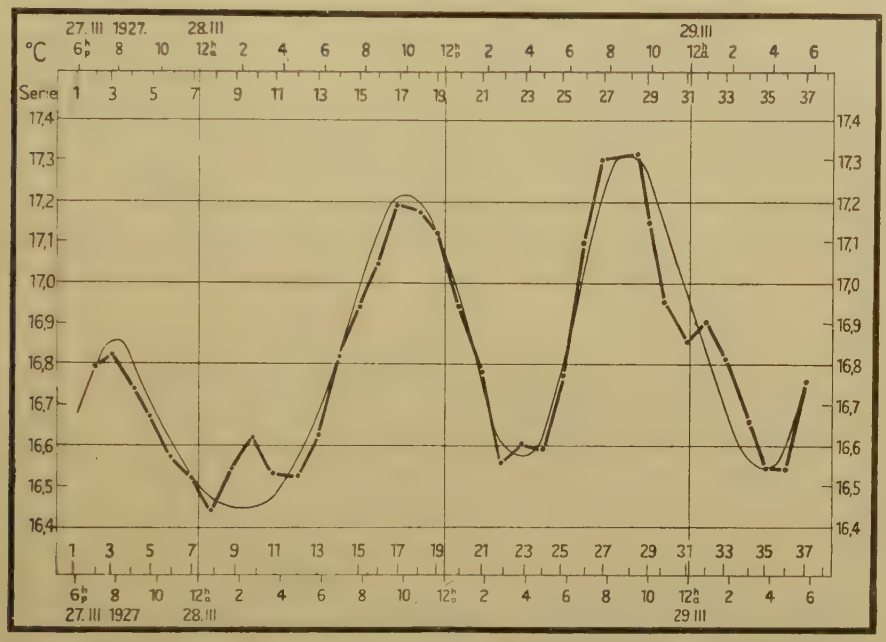

Fig. 9.

direction and magnitude of the velocity-component at a corresponding hour. Each lunar hour is indicated by a dot, and the passage of the moon — the upper passage in the case of the diurnal waves — is indicated by a ⊙. (On some of the curves the lower passage of the moon is indicated by a ×.) The direction of rotation of the current was everywhere *cum sole*, as is indicated by the arrows.

Fig. 10 represents in this manner conditions at our Station D, north of the Canaries on 30° latitude, where observations were pursued for nearly 6 days, whereas Fig. 11 refers to observations (over $3^1/_2$ days) at our Station E, 3 degrees farther west along the same parallel.

I wish now to call your attention to such peculiarities in these figures that seem to indicate the existence of internal wave-motion. Even if there were no internal waves, the water-layers might of course have an oscillating motion, but in this case the ampli-

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[1]) Furthermore the average velocities in the various levels are represented by arrows, but we are not concerned with them for the present purpose.

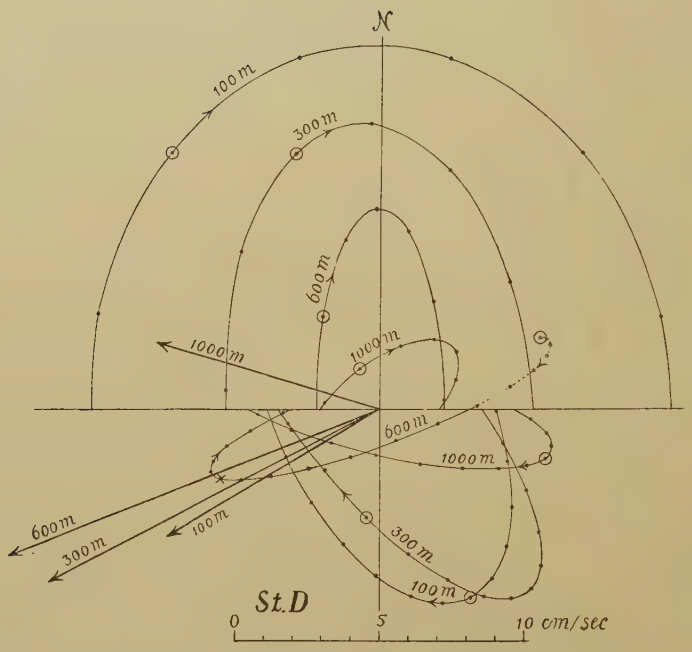
N
100 m
300 m
600 m
1000 m
1000 m
600 m
1000 m
600 m
300 m
100 m
300 m
100 m
St. D
0
5
10 cm/sec

Fig. 10.

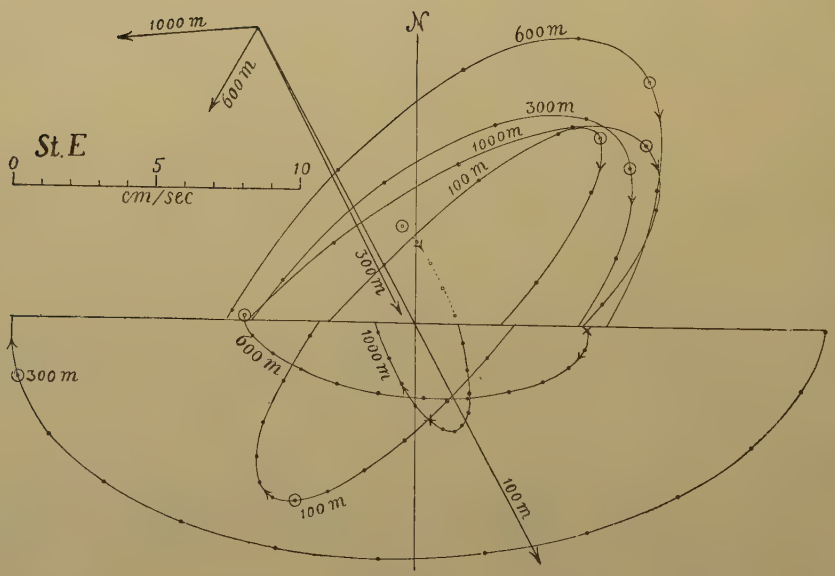
1000 m
600 m
St. E
0
5
10
cm/sec
N
600 m
300 m
1000 m
100 m
300 m
600 m
1000 m
300 m
100 m
100 m

Fig. 11.

tude and the phase should both be equal at all levels, except in the neighbourhood of the sea-bottom, where they should vary owing to friction.

In the case of the semidiurnal oscillations represented by Figs. 10 and 11 the phase and the orientation of the ellipses are approximately the same at all levels, but the amplitude is variable with a maximum in the neighbourhood of 600 metres at Station E, and particularly at Station D the great increase in amplitude towards the upper layers is obvious, so that the existence of semidiurnal waves must be considered as clearly established in this case.

Turning to the diurnal components the varying orientation of the ellipses and the varying phase should be noticed, and particularly there is at Station E a very pronounced maximum of amplitude in the neighbourhood of 300 metres, from which it seems necessary to infer a diurnal internal wave-motion. — Though I confess that I do not see how to bring it into agreement with the theoretical considerations which I have laid before you. For at the latitude of Station E the observed period would be very nearly 12 pendulum hours, and a wave-motion in the strict sense of the word would therefore be impossible[1]).

Difficulty of Explaining Tidal Periodicity.

Other instances could be added, and it seems impossible to deny the reality of tidal internal oscillations. But from a dynamical point of view it is difficult to understand them as a direct effect of tide-generating forces. The displacement of the gravitational equipotential surfaces in the sea owing to the sun's and the moon's gravitation is almost exactly equal at all levels, and from the point of view of the equilibrium theory all isodensal surfaces, the sea-surface included, should therefore perform equal vertical movements, so that no internal tidal waves at all ought to be possible.

According to the more relevant dynamical theory the tide-generating force has chiefly the effect of accelerating the water instead of keeping it in an altered position of equilibrium. But this accelerating effect is practically the same at all levels, and the tide-generating force has therefore little or no tendency to raise or maintain internal waves, in which we have seen that the acceleration has opposite directions below and

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[1]) Note to the printed report: Perhaps the described diurnal oscillations are not tidal waves at all, but free "inertial" motions, started by a wind or some other disturbing agency and then continuing by virtue of inertia alone. If it were so, they would afford an example of the limiting case of internal waves with a period of 12 pendulum hours. As a matter of fact it would be sufficient for the starting of such a motion within a particular water-layer that this water-layer obtained, within a comparatively large region of the sea, a uniform initial velocity.

Certainly this explanation of the observed oscillations is not free from objections. For it involves that the water-particles should move in circular orbits, whereas the observed motion was elliptic. With a wave-period smaller than 12 pendulum hours ($s < 1$) the axis of the elliptic orbits are not necessarily in the ratio s, as was stated on p. 8, if namely two trains of waves with equal periods are crossing oneanother in two directions; but in the limiting case $s = 1$ the orbits must necessarily be circles. Irrespective of this difficulty, however, the observed motion of the uppermost water-layers would seem to suggest the above-mentioned explanation (see V. W. Ekman and B. Helland-Hansen 1931).

above the boundary-surface. Moreover the wave-lengths of internal waves with tidal periods — in any case with semidiurnal tidal periods — are always very small as compared to the earth's radius, and since the tidal force is therefore very nearly the same over a wave-crest and the following wave-trough, its wave-generating effect will be correspondingly further reduced.

The only imaginable way in which tidal forces might directly generate and maintain considerable internal waves would therefore be by resonance with standing internal waves of very nearly the same period. For this possibility, however, it is necessary that the decrement of the free, standing waves should be very small — a condition which can hardly be fulfilled, not only because of the eddy-viscosity, but also because the standing waves would be incessantly dissipated by reflection at the irregularly shaped coast-lines.

In the case of waves of fortnightly and longer periods conditions are still more unfavourable, not only owing to the minuteness of the corresponding tidal forces, but also owing to the theoretical non-existence of free waves of these periods, to which I have already called your attention.

It might also be possible that the observed oscillations are only an indirect effect of the tidal forces, and two explanations of this kind have been proposed. On the one hand they may be "secondary boundary-waves", such as were discovered and studied by Prof. Zeilon in the Gullmar fjord, where they attained a height of half a metre. In the ocean they would be generated by ordinary tidal currents running over an uneven sea-bottom. In my opinion, however, this effect is not very likely to explain the observed internal waves of 10 or 20 metres in height, so much the more because conditions are in some respects less favourable than in the fjord. Thus, the distance from the sea-bottom to the boundary was 100 times as large at the Meteor-station 288 as in the Gullmar fjord, and furthermore in the open ocean the wave-motion would not be restricted to two dimensions but would spread in all directions.

Again Helland-Hansen (1930) has pointed out that within a region where the isodensal surfaces are sloping — that is within a field of solenoids — ordinary horizontal tidal currents will give rise to apparently vertical motions of the isodensal surfaces. It is possible that within the region of a strong field of solenoids apparently vertical oscillations of considerable magnitude may arise in this way. But they would hardly explain the oscillations at, for example, the anchor station 288 of the "Meteor". A semidiurnal current with a maximum velocity of as much as 26 centimetres per second would certainly not exist here. But even so strong a current would not communicate to the water greater displacements than 2 nautical miles, and to explain the observed tidal variations of temperature it would therefore be necessary to assume a horizontal temperature-gradient of 18 degrees in 60 miles, which is certainly impossible.

Puzzling Observations of Periodicity.

Still more puzzling is the observation by Helland-Hansen and Nansen (1909 and 1926) of similar waves, while the research-ship was performing a hydro-

graphic section. Such observations were made on several sections, and in some cases the oscillations seemed to have a diurnal tidal period, in other cases shorter periods as *e. g.* 6 or 10 hours. A case of apparently diurnal oscillations is illustrated by Fig. 12, which is borrowed from one of the cited papers (B. Helland-Hansen and F. Nansen 1926). The time is plotted horizontally, but the observations are taken at different stations, the numbers of which are indicated along the uppermost curve. Thus Station 7 lies off the southern end of the English Channel. From there the course was laid in a south-westerly direction to Station 15, which lies half way between the Channel and the Azores,

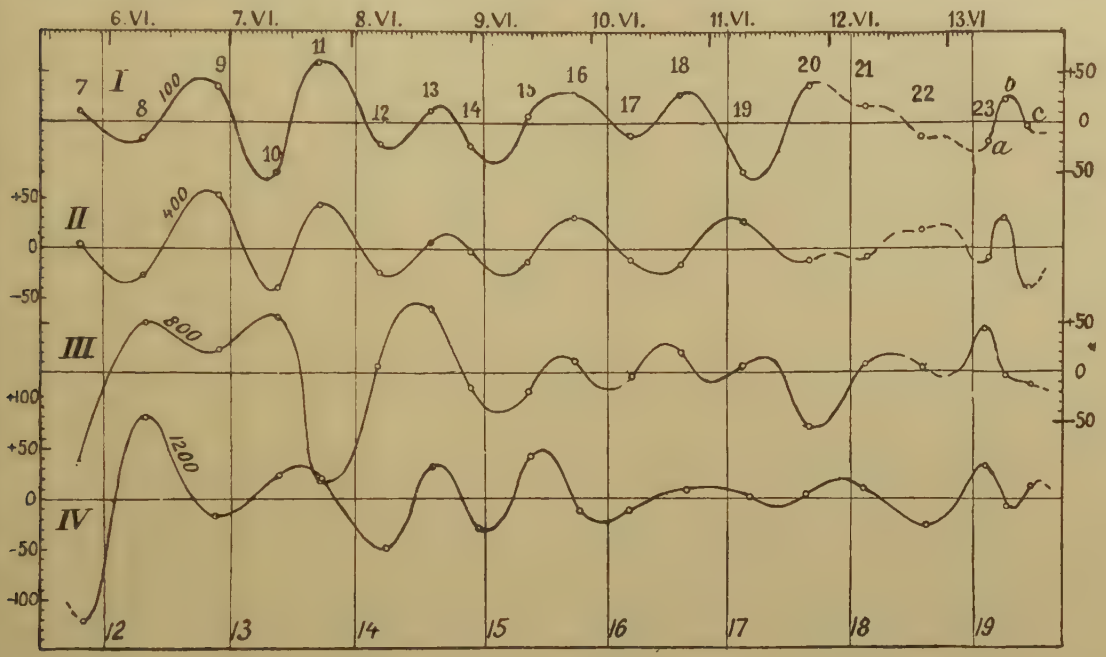

Fig. 12.

and from there towards south-east to Station 23 off Lisbon. The ordinates, reckoned from four different zero-lines for depths of 100, 400, 800, and 1200 metres, are calculated so as to represent the local differences in density only. Thus for instance the ordinate at Station 13 represents the density at this station, diminished by the average density at Stations 12, 13, and 14, and similarly for other stations. The vertical lines indicate intervals of 24 lunar hours. Particularly the two upper curves for 100 and 400 metres give the appearance of oscillations with a diurnal lunar period. But considering that the stations are situated along two lines at right angles to one another and with a total length of 800 miles it seems utterly impossible that these apparent oscillations could be really periodic. For several reasons progressive waves, standing waves, and horizontal displacements of the isodensal surfaces by means of tidal currents, are all equally unacceptable as explanations.

Now it should be remembered that we have to consider the single values only,

which are represented in the figure by small circles, and not the curves drawn smoothly through them. And considering that not more than two or three such values fall within the assumed period, it is perhaps not impossible to consider the variations in density as accidental and irregular. In this connection I wish to call your attention to the fact that this very method of studying the variations of a quantity by plotting differences

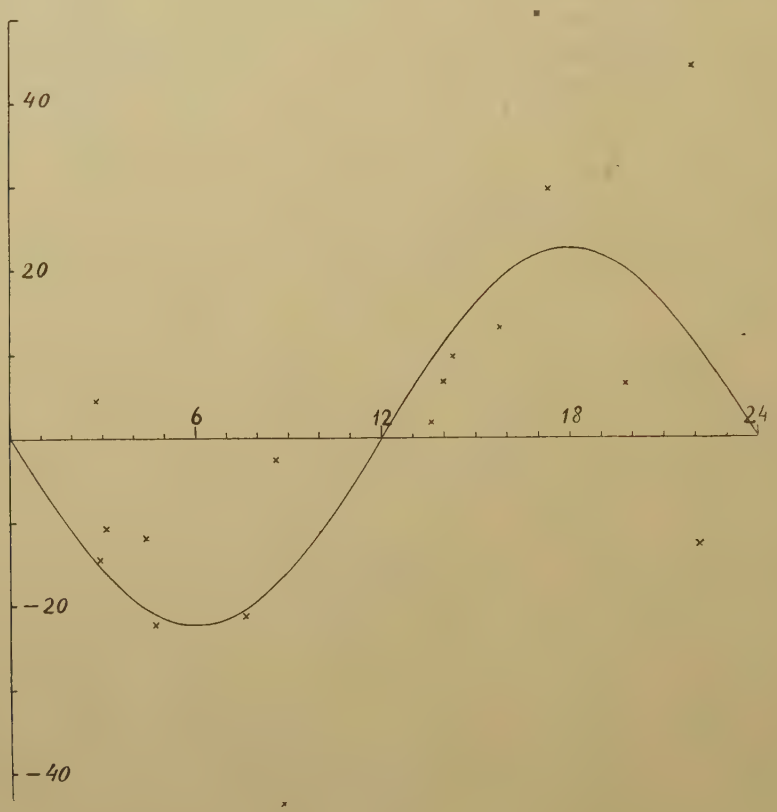

Fig. 13.

between single values and averages of three may produce to some extent an illusion of regular oscillations[1]). It is somewhat better to plot differences between single values and averages of five. I concede that there is, particularly in the two upper curves, a tendency towards a diurnal periodicity. This is best seen from a diagram, where the ordinates corresponding to stations 7 to 23 are plotted against lunar hours running from 1 to 24 only, irrespective of the day. For each station I have in this way plotted the mean value of the ordinates of the two upper curves in Fig. 12, and the result is seen in Fig. 13. It shows a rather decided indication of a diurnal period. But it should be

[1]) See in this connection p. 28—29 and Fig. 15.

observed that among HELLAND-HANSEN's and NANSEN's sections this seems to be just that one which is most in favour of the assumption of tidal periodicity. And since as I have already emphasized — the assumption is in this particular case utterly at

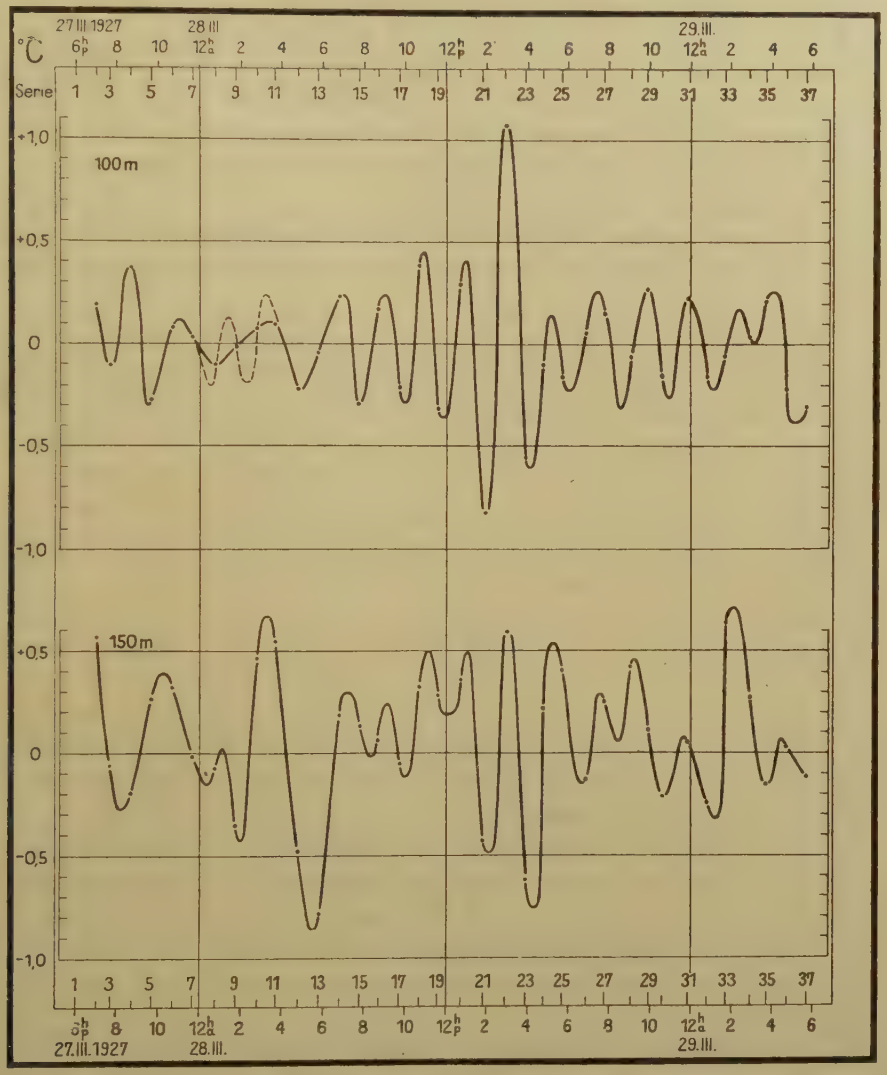

Fig. 14.

variance with rational principles, I think it is safest until further to regard the apparent periodicity as accidental and without real meaning, unless we unexpectedly find some explanation of which perhaps nobody is dreaming at present. Perhaps we shall be surprised one day by a simple explanation even of the tidal internal waves discovered at the anchor-stations already mentioned. But, I repeat, in this latter case the existence of the waves is a fact, which must be accepted irrespective of a satisfactory explanation.

Short-Period Waves.

We will turn once more to the remarkable series of observations at the anchor-station 288 of the "Meteor". This series is unequalled in its completeness and in the frequency of its observations, and the observed variations of temperature and salinity are extremely interesting. As my time is already drawing to a close, we must, however, confine ourselves to just one more side of the results, namely the short-period oscillations, which were eliminated from the tidal oscillations shown to you in Fig. 9.

These short period oscillations alone are seen in Fig. 14. The two curves, constructed for 100 and 150 metres depth, represent the individual temperature readings, each reading diminished by the average of three successive readings in the same way as in Fig. 12. They give an immediate impression of waves with a period of nearly 2 hours and a quarter. The amplitude is strongly variable, but it would not be difficult to con-

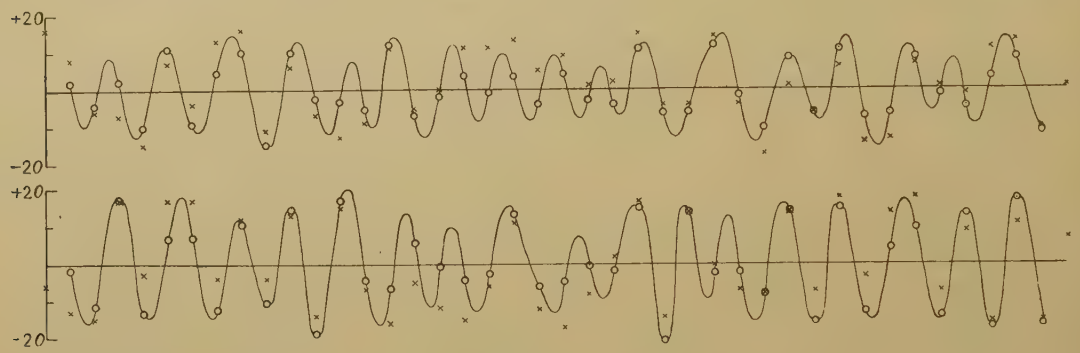

Fig. 15.

nect the given points by a wave-line with less variable amplitude. On the other hand they could just as easily be connected by a smooth but more irregular curve. The question therefore arises whether the periodicity of these rapid oscillations can be relied upon as real or not. As a matter of fact one can easily run the risk of being mistaken, if one deduces a periodicity from observations of which there are only 2 or 3 during each period. The objection is the same as in the case of the apparent waves which Helland-Hansen and Nansen (1926) found along the section from the English Channel to Lisbon; but the risk is of course increased if, on drawing the curves, small alterations of wave-period or phase are permitted in the middle of the series to be examined.

To make this matter clear I made the following experiment[1]), which is illustrated by Fig. 15. By casts of dice a series of 42 integers were taken at random, the numbers lying between the limits —17 and +18. These numbers are represented in the upper diagram by the ordinates of the signs ×, plotted at exactly equal horizontal distances. Then their "local variations" were calculated as differences between the respective integers and the average values of three consecutive ones. These "local variations" are represented in the diagram by small circles. The increased impression of a seeming periodicity is,

[1]) The experiment was briefly mentioned, but the figure was not shown, when reading the report.

in a manner, expressed by the fact that when passing from one number to the next there is a change of sign in 22 cases out of 41 when the original numbers are considered but in 29 cases out of 39 when the "local variations" are considered. The experiment was once more repeated (see the lower diagram), but this time the original numbers happened to change sign more frequently than is on an average to be expected. Wave-profiles were now drawn as smoothly and regularly as possible through the circles (see the figure), and we will imagine that the ordinates of the $\times$-marks represent fictitious temperatures observed at the times represented by the horizontal abscisses. It is perhaps worth noticing that in both diagrams the average "wave-period" thus represented happened to be about $2^1/_4$ times the interval of "time" between two consecutive "observations", exactly as in the real case of short-period oscillations represented by Fig. 14. And furthermore I think it must be agreed that the wave-profiles shown in Fig. 15 are not less regular, and the periodicity not less convincing, than in the case of Fig. 14.

By this criticism I do not want to positively deny the reality of the oscillations represented in Fig. 14, and of course their periodicity might be accepted if a natural dynamical explanation could be found for it. But I cannot find any, since no wave-generating forces of the period in question are known. Periodic waves may after the pattern of wind-waves, be generated by the upper water-layer running as a current on top of the heavier water; but this explanation would not do, because — considering the wave-length — an impossible velocity of the current (several metres per second) would be required.

Again one might imagine that the waves have arrived from a distant part of the sea and, like the swell after a gale, grown more and more regular on their way to the place of observation. This is all right in so far as the waves may have travelled a considerable distance. From the equation (8) or (9), p. 11 it may be calculated that the wave-amplitude, during one wave-period, would be diminished by only $0.004\sqrt{\mu}$ times its actual value. This means a very slow decay, all the more since the value of μ ought to be unusually small in consequence of the very sharp discontinuity-layer. But the explanation is nevertheless impossible. For, as you will remember, waves of the long-wave type, like those in the present case, travel along without any alteration of their wave-profile. Strictly this statement should be modified owing to the earth's rotation, for it does not hold when, according to equation (5), p. 9, the group-velocity is markedly smaller than the velocity of propagation, *i. e.* when the factor $\sqrt{(1-s^2)}$ is markedly different from 1. In the present case, however, (latitude 12°.5; assumed wave-period $2^1/_4$ hours) we have $s = 0.04$ only, so that the said factor $\sqrt{(1-s^2)} = 0.9992$ was very nearly equal to 1. The gradual alteration of the wave-profile of the progressive waves must therefore have been very slow.

Since the wave-length corresponding to the assumed wave-period should have been only 10 or 15 kilometres and the oscillations took place in the wide ocean, it seems equally impossible to explain the discussed oscillations as standing waves. On the whole I cannot see any reasonable theoretical ground for periodicity of the oscillations in question, and since the observations are not conclusive on this point, I am most inclined to regard the observed internal motions as an irregular splashing, resulting from

gusts of wind and other disturbing effects occurring here and there at greater or lesser distances away. Out from this view one should presume that the short-period oscillations might have shown an entirely different aspect if it had been possible to reduce the intervals of observation to a fraction of an hour.

To these remarks, which I had written before I came here, I wish to add that Professor DEFANT showed me last night other remarkable observations, made at the same station, on account of which it seems necessary to accept the existence of really regular wave-motions with various short periods, even if it seems impossible at present to understand them from a dynamical point of view.

If I am right in my views, with which I have already kept you too long, my survey may be concluded by stating that the important investigations on internal waves of the last 20 or 30 years have chiefly raised questions rather than solved them. We want very badly not only a fuller empirical knowledge of the matter but also a real understanding of the dynamical causes of the waves. In both directions there is — even on essential points — very much left to do.

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The Chairman, Prof. Knudsen thanked Prof. Ekman for his valuable contribution to the clearing up of the questions about internal waves and pointed out that this subject from the very beginning since the investigations of Professor Otto Pettersson had increased enormously in importance and now was the main point in modern hydrography. He then opened the

DISCUSSION

Director **van Riel** showed some results of the investigations on internal waves, made in the East Indian Archipelago during the Snellius-Expedition.

At the anchor-station 308a, in the Celebes Sea, temperature observations have been made at the surface, 50, 100, 150, 200, 250, 300, 400 and 600 m., approximately every hour for 27 hours consecutively.

Illustrating his remarks by curves, representing the temperature variations in different levels he said that:—

Besides the short irregular waves a nearly semi-diurnal period can be clearly seen at depths between 100 and 250 m., whilst at 600 m., where the smaller waves hardly appear, a period of a nearly diurnal character predominates.

The moments of occurrence of maximum and minimum values correspond in the various layers for the shorter and longer periods.

The amplitude in the upper layers is greatest in 150 m. for both long and short periods and decreases upwards to the surface and downwards to a depth of 250 m. In greater depths, however, the curves show a considerable increase of the amplitude of long and short waves.

The increase of the amplitude and the appearance of another period (nearly 24 hours) may point to another factor, operating in the deeper layers.

A maximum current at the surface coincides with minimum temperatures at 100 and 150 m.

Professor **Zeilon**:— I would like to mention one or two points on which my opinion differs from that of Prof. EKMAN. I do not think, though, that the disagreement is very serious. Prof. EKMAN said that the periodic character of the slow internal movements in the Gullmar Fiord ought to appear if their record for a long stretch of time were analysed harmonically. I do not think that such must necessarily be the case. The phenomenon which I have called "secondary internal seiches" shows that internal fluctuations exist which, though due to a definitely periodic agency, are not apt to be rendered visible by the harmonic analysis of a long record. In this very simple case the hydrodynamic explanation is, I think, clearly understood. The period of the secondary phenomenon is determined by the ordinary seiche-current which, when pent up by hindrances on the bottom of the fiord, produces disturbances in the boundary layer in the form of free boundary waves. But these secondary effects are not always visible; to appear with any considerable amplitude they demand a hydrographic situation whose density distribution is, as it were, favourably related to the bottom configuration of the fiord. As a matter of fact, even if the surface seiche is almost continually going on, the harmonic analysis of the secondary effects would as a rule be impossible, since their period only is determined by the primary agency, whereas their amplitude, wave-length and phase will be dependent of the continually changing features of the hydrographic situation.

My other point is this. I am doubtful about the validity of Mr. SVERDRUPS argument concerning the non-existence of very long waves in a rotating infinite basin. The argument is that the velocity of propagation for waves of periods above a certain critical value becomes imaginary; hence there would no longer be any waves. On the other hand, the fiction of an infinite basin evidently has no practical value unless representing a basin whose dimensions, though finite, are very large compared with the wave-lengths considered. Unfortunately, at the critical point where it passes from real to imaginary values, the velocity becomes infinite; hence the wave-length of ordinary as well as of boundary-waves becomes infinitely great too. This can to me have no other meaning than that the conditions imposed upon the problem are no longer fulfilled. Hence it seems difficult to admit that Mr. SVERDRUP'S conclusion has any bearing upon the tides of the earth.

Professor **A. Defant** showed a periodogram from an anchor-station near the Brasilian coast from the Meteor-Expedition. The ship stayed at the station for two

days and observations of the direction of the ship were made every 10 minutes and of temperature every hour.

The existence of periods of 1.9, 2.8 and 5.6 lunar hours (M_2) had been detected besides the diurnal and semidiurnal period.

These periods might be accounted for by an accidental wave which under the existing conditions was arising from some disturbing influence, but it could not be taken for granted that it would be possible to find these conditions again by another investigation at the same place.

Professor **Ekman**[1]): Professor Zeilon has referred to the "secondary boundary-seiche" as an example of a phenomenon the occurrence of which — though due to a definitely periodic agency — cannot be confirmed by harmonic analysis of a long record. To examine his argument I will assume with Prof. Zeilon that the primary surface-seiche really remains strictly periodic through months and years, while the internal stratification is altering. I think Prof. Zeilon will agree that the consequent alterations of amplitude of the secondary waves would not prevent their discovery by the means of harmonic analysis. On the other hand it is true that alterations of phase, and therefore indirectly alterations of wave-length, might do so. However, I am not disposed to admit that the said example is altogether relevant to the problem now under discussion. For if the absolute failure of the harmonic analysis should be explained as due to varying phase-differences — which in their turn can only be explained, according to Prof. Pettersson's theory, as a result of varying time of propagation of the waves from their place of generation to the place of observation — it would be necessary to assume variations of phase-difference as large as at least half a period, *i. e.* a week; which must certainly be excluded in the present case.

But even if the theory admitted the possibility of large phase-differences between the internal waves and the wave-generating force, I think our final conclusion must still be the same. For it is just the very occurrence of coincidences between internal wave-crests and maxima of the tide-generating force (without phase-difference) which should, according to Prof. Pettersson, furnish evidence of the presumed causal connection. If, owing, to the failure of the harmonic analysis, the phase is considered a matter of no essential consequence — which I understand to be the real purport of Prof. Zeilon's argument — there is then nothing left of the empirical support for the cosmic theory, except the casual occurrence of fortnightly intervals (alternating with other intervals of the most varying duration) between two or three successive wave-crests. This, however, is certainly not very strong evidence for the theory.

As to Sverdrup's theory Prof. Zeilon is right in observing that an endless ocean was assumed, and it is for this and other reasons obvious that the theory cannot hold exactly; but I think it may be safely used with the restrictions which I have already mentioned in my report. Regarding particularly the theoretical non-existence of long-period waves (which I have deduced independently of Sverdrup, though considerably

[1]) For the purpose of publication I have, with Prof. Zeilon's approval, taken the liberty of following up and amplifying the remarks which I actually made in the verbal discussion.

later than he), Prof. ZEILON's criticism is formally correct and may in a way invalidate Prof. SVERDRUP's mathematical proof, in so far as the general type of motion assumed is strictly a fiction; but it does not invalidate the theorem itself, which has a simple and independent explanation. For when the question is of progressive motions of long duration — say, of more than 12 pendulum hours, — the water can be regarded as devoid of inertia, and without inertia wave-motion, as well as any other free oscillating motions, are of course impossible. Likewise the infinite wave-length — and the infinite velocity of propagation — of waves with a period of 12 pendulum hours has an extremely simple explanation. Owing to the earth's rotation a particle which is left to itself will describe a circular path in the course of 12 pendulum hours; and a "wave-motion" with that period simply means that the water of the whole endless ocean — or a horizontal layer of it — moves as a rigid body without any change of surface-level, in such a manner that all water-particles describe simultaneously equal circular paths. On account of the coast-lines and the differences of latitude such a motion cannot of course be exactly realized, but a motion of approximately that type may very well happen to occur within part of the open ocean, so that there is nothing mystical about it.

In conclusion I wish to thank Professor ZEILON for his interesting remarks, which have brought further elucidation of the problem on important points.

Dr. **J. P. Jacobsen** mentioned that on a basis of the material from Danish light-ships and from some continuous measurements in the Langelandsbelt in June 1909 it was stated that internal waves were found at many places in the Cattegat and in the Belt Sea. It was found that the amplitude of these internal waves could on an average be 1 or 2 metres and that the phase of these waves did not for the different places show any simple correlation to the phase of the ordinary simple semidiurnal tidal wave. He thought that the explanation of these internal waves should be sought in the circumstance that the ordinary tidal wave by moving through the Cattegat would find different obstacles for its progression. Then internal waves could arise from these obstacles and in propagating come to the places where they were observed with different phases (see J. P. JACOBSEN: Medd. fra Kommissionen for Havundersøgelser, Vol. II, No. 2, page 84).

Professor **N. Zeilon** said that for the internal waves in the Cattegat the explanation given by Dr. JACOBSEN was probably true.

When the discussion on internal waves had finished, the question of the programmes for the hydrographical scientific meeting next year was dealt with (see below).

REPORT

Internal Waves and Turbulence in a Fluid of Variable Density.

By

G. I. Taylor.

If a small disturbance is imposed on a fluid of variable density which is otherwise at rest, it will resolve itself into a number of internal waves which will be propagated away from the centre of disturbance. On the other hand in a homogeneous fluid the various layers of which are in relative motion, experience seems to show that initial disturbances increase. In most motions in the atmosphere and in the sea both stratification of density and relative motion of the various layers co-exist. The dynamical effect of such systems is here considered in relation (a) to internal waves, (b) to the diffusion of properties like salinity and temperature, and the diffusion of momentum.

Internal waves.

Taking the case of a fluid possessing a small continuous increase in density downwards equal to $\beta\varrho = \frac{d\varrho}{dz}$, and a uniform rate of increase in velocity, $\alpha = \frac{du}{dz}$; then in the case when the upper boundary is a horizontal plane surface stable internal waves of all lengths can exist if $\alpha^2 < 4g\beta$. If $\alpha^2 > 4g\beta$ no waves either stable or unstable can exist. Thus the criterion[1])

$$\frac{\alpha^2}{g\beta} = 4 \qquad (1)$$

divides cases where waves can exist similar to those in a fluid at rest, but modified by the velocity gradient, and cases like that of a homogeneous fluid in which no waves can exist. No unstable waves can exist in such a fluid.

When the density is not continuous certain types of waves may become unstable, thus if there is an intermediate layer of fluid of small thickness h and density ϱ lying

[1]) Proc. Roy. Soc. A. Vol. 132 p. 499, Aug. 1931.

above a fluid whose density os $\varrho + \Delta\varrho$ which extends downwards indefinitely, and below a fluid of density $\varrho - \Delta\varrho$ which extends upwards, I find that if the whole system moves with a uniform rate of shear $\frac{du}{dz} = \alpha$ there are always some waves which are unstable. When α is small these are of very short wave length, but when α rises to a certain value long waves suddenly become unstable too. This critical value is independent of the wave-length and is given by

$$\alpha^2 = 2g\left(\frac{\Delta\varrho}{h\varrho}\right). \tag{2}$$

Evidently the quantity $\beta = \frac{1}{\varrho}\frac{d\varrho}{dz}$, which defines the rate of change in ϱ in the fluid of continuously varying density, is of the same nature as $\beta' = \frac{\Delta\varrho}{h\varrho}$ in the discontinuous problem now being considered; so that the criterion for stability of long waves, namely

$$\alpha^2 = 2g\beta' \tag{3}$$

is of the same type as (1). When the system contains two intermediate layers instead of one, each of thickness h, the criterion (3) becomes $\frac{\alpha^2}{g\beta'} = 2.10$. Possibly an infinite number of layers would bring the criterion up to $\frac{\alpha^2}{g\beta'} = 4$ so that the instability of such a discontinuous system might be regarded as corresponding exactly with the condition in the continuous case when no waves are possible, either stable or unstable.

These are definite problems in wave propagation designed to bring out the dynamical effect of density distribution. They are hardly likely to be of use to hydrographers.

Diffusion.

Though all infinitely small motions in a fluid seem to be stable when $\frac{\alpha^2}{g\beta} < 4$ this does not mean that all cases of flow for which $\frac{\alpha^2}{g\beta} < 4$ are non-turbulent. The steady flow of a viscous fluid through a pipe is apparently absolutely stable for infinitely small disturbances at all speeds, but it is well known that if the speed of flow exceeds a certain limit the motion becomes turbulent. In the case of a stratified fluid with density increasing downwards any disturbance to steady laminar flow necessarily involves an increase in gravitational potential energy. It is this increase which causes infinitely small disturbances of a fluid at rest to be stable. In cases of turbulent flow the rate at which energy flows into any layer of the fluid owing to the action of the fluid above and below it must be greater than the rate at which gravitational energy increases in the same portion of the fluid.

Let us try to express this principle in mathematical language. Starting with the fluid in its equilibrium state of uniform laminar motion and with a horizontal stratifica-

tion of density, any disturbance will cause the particles of fluid to wander out of their equilibrium level. If ζ is the vertical distance of a particle from its initial equilibrium position the increase in gravitational potential energy per unit volume due to the disturbance is $\frac{1}{2} g\beta\varrho\overline{\zeta^2}$, where the bar denotes that a mean is taken over a large horizontal area. The rate at which gravitational energy is increasing is therefore $\frac{1}{2}\varrho g\beta \frac{d(\overline{\zeta^2})}{dt}$.

The rate at which energy flows into unit volume owing to the eddies in the surrounding fluid can only be expressed by means of integrals which cannot be evaluated without a knowledge of the actual structure of the eddies themselves; but if it is assumed as has been done by Reynolds, Prandtl and others that the whole dynamical effect of the eddies on the mean motion can be represented by means of a virtual coefficient of eddy viscosity which may be represented in the notation of Dr. Jacobsen by the symbol $\varrho\mu_u$, then the energy communicated to unit volume can be calculated as follows:— T, the horizontal friction force per unit area, which may be called the Reynolds stress, is, by the definition of μ_u, equal to $T = \varrho\mu_u a$ where $a = \frac{du}{dz}$. The work done by the Reynolds stress and the pressure gradient per unit area of a horizontal layer of thickness δz is $Ta\delta z$ or $\varrho\mu_u a^2\delta z$, so that the mean rate at which energy enters unit volume is $\varrho\mu_u a^2$.

If all this work is used in increasing the gravitational potential energy

$$\varrho\mu_u a^2 = \frac{1}{2}\varrho g\beta \frac{d(\overline{\zeta^2})}{dt}. \tag{4}$$

In a fluid possessing some transferable property, S, like salinity or temperature, the rate at which S crosses any horizontal area, A, is $Av\overline{\zeta}\frac{dS}{dz}$ where v is the vertical component of velocity at any moment. Now[1]) $\overline{v\zeta} = \frac{d(\frac{1}{2}\overline{\zeta^2})}{dt}$ so that $A\overline{v\zeta}\frac{dS}{dz} = A\frac{dS}{dz}\frac{d(\frac{1}{2}\overline{\zeta^2})}{dt}$. In the notation of Dr. Jacobsen this is defined as being equal to $A\mu_s\frac{dS}{dz}$ so that $\mu_s = \frac{1}{2}\frac{d(\overline{\zeta^2})}{dt}$ and (4) becomes

$$\frac{a^2}{g\beta} = \frac{\mu_s}{\mu_u}. \tag{5}$$

If some of the energy supplied by the Reynolds stress T is dissipated by viscosity it will be seen that $\frac{\mu_s}{\mu_u}$ must be less than $\frac{a^2}{g\beta}$, which is therefore an upper limit to the value of $\frac{\mu_s}{\mu_u}$. In the case where $\frac{a^2}{g\beta}$ is considerably less than unity we might expect the

[1]) See "Diffusion by continuous movements", Proc. Lond. Math. Soc. 1920.

rate of dissipation of energy due to viscosity to be small compared with the rate of increase in potential energy so that (5) would hold. On the other hand when $\frac{\alpha^2}{g\beta}$ is great compared with unity, as it is in the case of a fluid of uniform density, there is little or no change in gravitational potential energy due to turbulence so that the value of $\frac{\mu_s}{\mu_u}$ must be determined from other considerations.

Arguments of this type were first given, so far as I am aware, by L. F. RICHARDSON[1]) in 1920 who mentioned in a note to his paper that I had also used the same argument. At that time, however, we both introduced the additional assumption first made by REYNOLDS and later independently by PRANDTL and others that momentum is transferred through a fluid by means of turbulence in exactly the same manner as temperature, salinity or other transferable properties. This assumption is expressed by the condition

$$\frac{\mu_s}{\mu_u} = 1.0. \tag{6}$$

If this assumption is made then the conclusion which must be drawn from the preceding argument is that if $\frac{\alpha^2}{g\beta}$ is less than 1.0 the energy communicated to any layer by turbulence is not sufficient to supply the necessary increase in gravitational potential energy so that turbulence cannot be maintained. In other words the criterion $\frac{\alpha^2}{g\beta} = 1.0$ divides notions in which turbulence cannot be maintained from those in which it can.

Recently PRANDTL[2]) has independently used identical ideas, but has obtained the criterion $\frac{\alpha^2}{g\beta} = \frac{1}{2}$ instead of 1.0. It is of interest therefore to inquire wherein the difference lies.

To calculate the rate at which work is done against gravity PRANDTL imagines a fraction β_1 of the fluid to be moving upwards with velocity v_1 and the remainder, β_2, to be moving downwards with velocity v_2. He then defines a mixture-length "l" as the mean length in a vertical direction through which any particle of fluid moves after detaching itself from one layer till it mixes with another. With this definition the potential energy in the fluid which crosses a horizontal area A in one second is

$$A(\beta_1 v_1 + \beta_2 v_2)\left(\frac{1}{2} g l^2 \frac{d\varrho}{dz}\right); \tag{7}$$

z being measured upwards. He then obtains as the work done on a layer of thickness l and area A the expression

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[1]) L. F. RICHARDSON. Proc. Roy. Soc. A. Vol. 97 p. 354.

[2]) Vorträge aus dem Gebiete der Aerodynamik und verwandten Gebieten. Aachen 1929.

$$A T l \frac{du}{dz} \tag{8}$$

where T is the Reynolds stress $T = \varrho \overline{u'v'}$ and $u'v'$ are the components of eddy motion in a horizontal and vertical direction at any point. Assuming then that

$$u' = l \frac{du}{dz}, \qquad v' = \beta_1 v_1 + \beta_2 v_2. \tag{9}$$

PRANDTL thus obtains the expression

$$\varrho \left(\frac{du}{dz}\right)^2 > \frac{1}{2} g \frac{d\varrho}{dz} \text{ or } \frac{a^2}{g\beta} > \frac{1}{2} \tag{10}$$

for the condition that the work done by the Reynolds stress must be greater than the work done in increasing gravitational potential energy.

It will be seen that this argument is identical with that given above except that PRANDTL makes use of a "mixture length", l, which does not come into our analysis or into the final result. The difference between PRANDTL's result and ours must arise from his use of the mixture length l. The use of a mixture length as an intermediate conception should not invalidate the result even though l does not occur in the final expression for the criterion $\frac{a^2}{g\beta} = \frac{1}{2}$, but it seems to me that a definite mathematical error has arisen in its application when it is assumed that the "l" which occurs in (7) is identical with the "l" of (9).

In order that expression (7) may be correct "l" must be defined as the mean length in a vertical direction through which any particle of fluid moves after detaching itself from one layer till it mixes with another. In order to find the Reynolds stress $T = \varrho \overline{u'v'}$ the mean value of $u'v'$ must be taken over a horizontal plane (or indeed through any assigned volume). The value of u' used in this product must therefore, if PRANDTL's principle be used, be $l_1 \frac{du}{dz}$, where l_1 is the mean vertical distance through which a particle has moved since it started its path at a level where it had the same horizontal velocity as the mean velocity of that level. The mean value of l_1 is clearly less than l because at any instant when $\overline{u'v'}$ is estimated all values of l_1 between 0 and l must be expected. It cannot be assumed as is done by PRANDTL that all the particles considered in estimating the value of $\varrho \overline{u'v'}$ are at the extreme limit, l, of their paths. On the contrary l_1 must be only $\frac{1}{2} l$ so that $u' = \frac{1}{2} l \frac{du}{dz}$. Using this value for u' the criterion becomes $\varrho \left(\frac{du}{dz}\right)^2 = g\left(\frac{d\varrho}{dz}\right)$ or $\frac{a^2}{g\beta} = 1$, which is identical with RICHARDSON's formula. It seems to me therefore that the difference between our criterion $\frac{a^2}{g\beta} = 1$ and PRANDTL's criterion $\frac{a^2}{g\beta} = \frac{1}{2}$ is due to a mathematical error rather than to a difference in principle.

The method of treating turbulence developed in my paper on Diffusion by Continuous Movements depends on the idea of correlation between the motion of a particle at one instant and that at later times. The average vertical distance through which a particle moves during the period necessary for the correlation between its velocity at the beginning and end of its path to disappear might perhaps be related to PRANDTL's "l" but this length does not enter into the analysis which yields the formula (5).

Comparison of condition $\frac{\mu_s}{\mu_u} < \frac{\alpha^2}{g\beta}$ with hydrographic observations.

The only observations of which I am aware in which all the relevant quantities have been measured are those of Dr. JACOBSEN at Schultz's Grund[1]) and Randers Fjord[2]). By measuring the distribution of density and current velocity at various depths at two stations in regions where a current of fresh water flows over a salt current Dr. JACOBSEN was able to calculate both the intensity of mixing and the friction at various depths in both these cases. His results are summarised in Table I for Schultz's Grund and in Table II for Randers Fjord. In each table the col. 1 gives the depth, col. 2 the measured value of $\alpha = \frac{du}{dz}$, col. 3 gives $\frac{ds}{dz}$ where s is the salinity, col. 4 gives $\beta = \frac{1}{\varrho}\frac{d\varrho}{dz} = 7.5\times10^{-4}\frac{ds}{dz}$ col. 5 gives Dr. JACOBSEN's calculated value of μ_s and col. 6 his calculated value of μ_u Col. 7 gives the values of $\frac{\mu_s}{\mu_u}$ and col. 8 gives the values of $\frac{\alpha_2}{g\beta}$ taken from cols. 2 and 4.

The first point which strikes one in these tables is that $\frac{\mu_s}{\mu_u}$ is always very much less than 1.0 which is the value it would necessarily have if the PRANDTL-REYNOLDS theory of the relationship between momentum and heat or salinity transfer were true. This has been pointed out by Dr. JACOBSEN. The second point is that at all depths except those close to that at which $\frac{du}{dz} = 0$, $\frac{\alpha^2}{g\beta}$ is of the same order of magnitude, but greater than $\frac{\mu_s}{\mu_u}$. The third point is that the actual values of μ are always more than 100 times as great as the coefficient of viscosity of water so that the friction between superposed layers of water must be due to turbulence, yet both $\frac{\alpha^2}{g\beta}$ and $\frac{\mu_s}{\mu_u}$ are always much less than 1.0.

It appears therefore that turbulence can persist when $\frac{\alpha^2}{g\beta}$ is much less than 1.0 and that this is connected with the fact that the PRANDTL-REYNOLDS theory that $\mu_u = \mu_s$ is untrue, the diffusion of momentum in a stratified fluid being far more rapid that the diffusion of salinity or temperature. On the other hand the formula $\frac{\alpha^2}{g\beta} > \frac{\mu_s}{\mu_u}$ appears to be approximately true except near the layer where $\alpha = 0$.

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[1]) Beitrag zur Hydrographie der Dänischen Gewässer 1913.

[2]) Rapp. et Proc.-Verb. des Réunions du Conseil Perm. Inter. pour l'Exploration de la Mer, Vol. LXIV p. 61.

In recent years a great deal of work on the relationship between μ_s and μ_u has been done in the field of Aeronautics. The assumption of REYNOLDS and PRANDTL that $\mu_u = \mu_s$ has been confirmed in all cases where the fluid is homogeneous and if the theory is true it must apply equally well to a fluid of variable density as to a homogeneous fluid. For this reason I was at first unwilling to accept Dr. JACOBSEN's conclusion that

Table I. Data for Schultz's Grund.

1 Depth metres	2 $\frac{du}{dz} = \alpha$	3 $\frac{ds}{dz}$	4 $\beta = 7.5\times10^{-4}\frac{ds}{dz}$	5 μ_s	6 μ_u	7 $\frac{\mu_s}{\mu_u}$	8 $\frac{\alpha^2}{g\beta}$
2.5	-10×10^{-3}	10×10^{-4}	7.5×10^{-7}	0.3	3.1	0.09	0.14
5.0	—17	15	11.2	0.4	3.1	0.13	0.26
7.5	—22	38	28.5	0.18	2.7	0.067	0.17
10.0	—24	80	60.0	0.05	2.2	0.023	0.098
12.5	—19	140	105.0	0.04	1.9	0.021	0.035
15.0	—8	110	82.5	0.2	3.8	0.05	0.008
17.5	0	62	46.5	0.74			
20.0	+4	30					

Table II. Data for Randers Fjord.

1 Depth metres	2 α	3 $\frac{ds}{dz}\times10^2$	4 β	5 μ_s	6 μ_u	7 $\frac{\mu_s}{\mu_u}$	8 $\frac{\alpha^2}{g\beta}$
1	—.033	0.9	0.67×10^{-5}	0.6	3.5	0.17	0.17
2	—.071	1.8	1.35×10^{-5}	0.5	2.5	0.20	0.38
3	—.062	2.0	1.50×10^{-5}	0.4	2.6	0.15	0.26

in the sea μ_u may be many times as great as μ_s, and I examined his observations with a view to finding out whether the discrepancy might be due to neglect of certain terms in the hydrodynamical equations. Though some error is undoubtedly introduced in this way I could not see the possibility of explaining such a large discrepancy as that between 0.2 and 1.0.

The idea latent in the formula (5) is that to different modes of disturbance correspond different values of $\frac{\mu_s}{\mu_u}$; but the existence of a density gradient operates so as to prevent the formation of modes of disturbance which are such that the rate of increase of gravitational potential energy in any region would be greater than the rate at which the Reynolds eddy stresses communicate energy to that region. It follows that this condition is equivalent to the statement that modes of disturbance for which $\frac{\mu_s}{\mu_u} > \frac{\alpha^2}{g\beta}$ cannot maintain themselves.

Lateral diffusion.

In meteorological observations it has been found that the existence of a stable temperature gradient in the atmosphere near the ground has a powerful effect tending to reduce the amount of turbulence in the lowest layers of the atmosphere. It seems that a stable distribution of density might be expected to suppress the vertical component of velocity in turbulent motion, but there is no obvious *a priori* reason why the two horizontal components i. e. the down-wind and cross-wind components should also be reduced. It is found, however[1]), that all the three components are reduced equally, so that if the turbulent velocity (i. e. vector difference between the mean velocity and the total velocity at any instant) is represented as a vector, the end of this vector moves about inside a sphere. This sphere is merely reduced in radius when a stabilising density gradient exists. It is not altered into an ellipsoid as one might have guessed. It appears therefore that the lateral diffusion is likely to be equal, on the average, to the vertical diffusion. In that case it might be of considerable interest to observe the scattering of a number of objects, such as weighted vertical floating rods, started in a current in the sea from a given point at successive short intervals of time.

The diffusion calculated by applying statistical analysis to the observed scattering could be compared with the value of μ_s derived from salinity measurements.

The Chairman thanked Prof. TAYLOR for his interesting report and opened the

DISCUSSION

Dr. **H. Thorade** asked what the reason was for the objection to the relation of PRANDTL:

$$\varrho \cdot \left(\frac{du}{dz}\right)^2 > -\frac{g}{2} \cdot \frac{d\varrho}{dz},$$

where ϱ is the density of the seawater, u the horizontal velocity, z the depth and g the acceleration.

Prof. **Taylor** answered that probably there was a mistake in the line of argument of PRANDTL about the path of a waterparticle when passing a definite area. If corrected the relation should be

$$\varrho \left(\frac{du}{dz}\right)^2 > -g \cdot \frac{d\varrho}{dz}.$$

Dr. **Jacobsen** said that possibly his method of procedure for finding the quantity μ_s should be applicable only for an ideal case which in reality could not exist in the

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[1]) "Observations and Speculations on the Nature of Turbulent Motion", Report and Memoranda Advisory Committee for Aeronautics 1917 and "Turbulence" Q. J. R. Met. Soc. 1927 p. 201.

nature. His attention was drawn to this circumstance by Prof. TAYLOR by correspondence, but he had not yet had an opportunity for a closer examination of the problem.

Prof. **V. W. Ekman** asked Prof. TAYLOR to give an explanation of the quantity which in the theory of the turbulence is replacing the mean free path in the theory of the molecular viscosity.

Prof. **Taylor**:— It is hardly the time to start a description of the theory of "Diffusion by continuous movements" which I published some years ago, but it is worth noticing that I have used it implicitly in the formula $\mu_s = \frac{1}{2}\frac{d}{dt}(\overline{l^2}) = \overline{vl}$. If μ_s is constant in regard to time $\overline{l^2}$ increases at a uniform rate and $\overline{vl}$ remains constant because as l increases its correlation with v decreases at just such a rate that $\overline{vl}$ remains constant. It is this principle which makes it possible to derive such formulae representing the effect of turbulence without considering the exact point in the path of a particle where "mixture" takes place.

Prof. **R. Witting** described an experiment which he had made in order to ascertain the propagation of waterparticles from one particular place in the sea. Some dye-stuff (fuchsine) had been dissolved in seawater so that the solution had nearly the same density as the seawater at the place where the experiment was carried out. By pouring the solution into the sea and observing the propagation of the dye-stuff some qualitative results were gained but quantitative results were not yet obtained.

REPORT

Mischung, Turbulenz und Grenzfläche.

Von

H. Thorade.

1. Entstehungsorte der Turbulenz. In einer kürzlich erschienenen Arbeit von B. Schulz (1930) wird ausgeführt, dass von dem ozeanischen Wasser, das bei Skagen dem zur Ostsee hin fliessenden Unterstrome beigemengt ist, nur ein Drittel die Höhe von Fornäs und noch nicht der dreizehnte Teil den Rand des Arkona-Beckens erreicht; der Rest fliesst mit dem Oberstrome wieder heraus, ohne in die eigentliche Ostsee tiefer eingedrungen zu sein; und nach J. P. Jacobsen (1925) gelangt der gleiche Unterstrom zwar bis zum Nord-, aber in der Regel nicht bis zum Südausgange des Öresundes; dies zwingt zu dem Schlusse, dass das in ihm enthaltene salzreiche Wasser in den Oberstrom übergeht und von ihm wieder hinausgeführt wird. Das Kattegat und die Beltsee stellen sich somit dar als eine Art Laboratorium für Mischungsvorgänge im Grossen. Was die Ursachen dieser hochgradigen Turbulenz anlangt, so könnte man sie entweder in Erscheinungen an der Oberfläche, oder am Boden, oder an der inneren Sprungfläche suchen, an der zwei Ströme von entgegengesetzter Richtung über einander hinweg fliessen.

Nun ist zum Entstehen der Turbulenz nicht nur nötig, dass der regelmässige Verlauf einer Strömung irgendwie gestört wird, — solche Störungen wird es im Meere wohl meistens geben —, sondern dass die Reynolds'sche Zahl

$$R = \frac{U \cdot h}{\nu}, \tag{1}$$

wo U die mittlere Geschwindigkeit, h die Wassertiefe und ν die kinematische Zähigkeit bedeutet, einen gewissen Mindestwert übersteigt, der ein wenig über 300 zu liegen scheint. In 30 m tiefem Wasser von 15° C, 35 $^0/_{00}$ Salzgehalt, und bei einer Stromgeschwindigkeit von nur 0,1 cm/sec wäre aber bereits $R = 25000$, und daraus ergäbe sich, dass es unter diesen Umständen eine turbulenzfreie Strömung praktisch nicht geben würde. Um so mehr müsste man nach (1) auf Turbulenz rechnen, wenn die Geschwindigkeit oder die Wassertiefe grösser ist; hervorzuheben ist vielleicht noch, dass R um so grösser wird, je kleiner die Zähigkeit ν ist, dass also die Turbulenz sich in Flüssigkeiten mit geringer

Reibung am stärksten entwickelt, und dass sie durch diese nicht so sehr begünstigt, als gedämpft wird. Im Meere freilich dürfte ein anderes Hemmnis der Turbulenz wichtiger sein, das von der stabilen Schichtung herrührt; denn nicht ohne einen Aufwand von Energie kann ein Wirbelkörper oder ein Flüssigkeitsballen in eine höhere oder tiefere Lage gelangen, als ihm nach seinem spezifischen Gewichte zukommt. Leider sind Beobachtungen hierüber äusserst spärlich, doch hat L. Prandtl (1930) die theoretische Forderung aufgestellt, dass

$$\varrho\left(\frac{dU}{dz}\right)^2 > \frac{g}{2}\frac{d\varrho}{dz} \qquad (2)$$

(z = Abstand vom Boden, g = Schwerebeschleunigung, ϱ = Dichte; der rechten Seite ist ein positives Zeichen zu geben) sein müsste, damit die Turbulenz nicht durch den Archimedischen Auftrieb unterdrückt werde, eine Formel, die sich bis auf den Faktor $^1/_2$ mit einer älteren von L. F. Richardson (1920, S. 364) deckt[1]). Wenn man nun aus der erwähnten Arbeit von J. P. Jacobsen (1925, S. 35) für das Feuerschiff »Schultz' Grund« die Zone der stärksten Geschwindigkeitsänderung entnimmt, so findet man etwa 19 cm/sec Abnahme und (S. 69 ebda.) einen Dichteunterschied = 0,011 auf einen Höhenunterschied von 10 m, womit sich (in Millionen) für die linke Seite der Prandtlschen Ungleichung 366, für die rechte aber nach Fortlassung des Faktors $^1/_2$ ungefähr 10800 ergibt. Danach würde die Verschiedenheit der Strömung ober- und unterhalb der Sprungschicht nicht entfernt ausreichen, um einen turbulenten Zustand zu erzeugen. Da ein solcher aber unzweifelhaft vorhanden ist, so ist sein Ursprung an der Oberfläche oder — wahrscheinlicher — am Boden zu suchen, und diese Ansicht findet eine Stütze in der Übersicht, die A. Defant (1930) über die verschiedenen Arten der Turbulenz gegeben hat. Es möge gestattet sein, im Folgenden auf die Vorgänge am Boden näher einzugehen, zumal über die Oberflächenturbulenz bisher nur wenige Anhaltspunkte vorhanden sind.

2. Turbulenz und Mischung. Man pflegt in der turbulenten Übertragung der Geschwindigkeit von einer Wasserschicht auf die nächste ein makroskopisches Gegenstück zur inneren Reibung zu erblicken, indem die Bewegungsgrösse im wesentlichen nicht mehr durch die Molekeln, sondern durch grössere Flüssigkeitsballen oder Wirbel verfrachtet wird: »Molare« Reibung nach O. Reynolds, »virtuelle« Reibung nach V. W. Ekman, »Scheinreibung« oder »Austausch« nach W. Schmidt (1926). Aber der Unterschied besteht nicht nur in einer tausend- und mehrfach höheren Grössenordnung der Scheinreibung gegenüber der molekularen Zähigkeit. Denkt man sich nämlich eine wagerechte Strömung von konstanter Richtung über ebenem Boden, deren mittlere Geschwindigkeit U mit dem Abstande z vom Boden zunimmt, deren Geschwindigkeitsgradient $\frac{dU}{dz} = \alpha$ also positiv ist, denkt man sich ferner, dass die Teilchen ausserdem gewisse mit der Zeit veränderliche Störungsbewegungen ausführen, deren wagerechte

[1]) Vgl. auch die Kritik G. I. Taylor's im vorhergehenden Vortrage, S. 38—40. Mit den dort gebrauchten Bezeichnungen würde die Beziehung lauten:

$$\frac{\alpha^2}{g\beta} > \frac{1}{2}, \text{ bzw.} > 1.$$

und senkrechte Komponenten u und w seien, so geht durch eine wagerechte 1 qcm grosse Fläche in der kurzen Zeit dt durchschnittlich ein Wasservolumen $= w \cdot dt$, also die Masse $\varrho w dt$, wenn ϱ wieder die Dichte bezeichnet. Die wagerechte Komponente der Geschwindigkeit ist $U + u$, die mitgeführte Bewegungsgrösse also, (d. i. das Produkt aus Masse und Geschwindigkeit) $= \varrho(U + u) w dt = \varrho U w dt + \varrho u w dt$. Von diesem Ausdrucke ist der Mittelwert für eine längere Zeit zu berechnen, was durch Querstriche angedeutet sein mag, $= \varrho \overline{Uw} + \varrho \overline{uw}$. Der erste Summand verschwindet, weil U konstant und der Mittelwert $\overline{w} = 0$ ist; denn die Bewegung hat ja keine dauernde Auf- oder Abwärtsströmung zur Folge; auch der Mittelwert $\overline{u}$ ist, für einen längeren Zeitraum berechnet, $= 0$, weil U die mittlere wagerechte Komponente der Geschwindigkeit ist; nicht aber verschwindet das Mittel der Produkte $u \cdot w$; dieses könnte nur $= 0$ werden, wenn die Störungen so regellos wären, dass innerhalb längerer Zeit ein jedes u immer mit allen möglichen positiven und negativen w-Werten verkoppelt vorkäme; denn dann hätte man eine sehr grosse Anzahl von endlichen positiven und negativen Produkten $u \cdot w$, die rein zufällig verteilt wären und sich im Mittel gegenseitig vernichten würden. Offenbar haben aber die in eine bestimmte Schicht von unten eingedrungenen Teilchen in der Regel eine geringere wagerechte Geschwindigkeit als diese Schicht, also ein negatives u (bei einem positivem w), und entsprechend wird sich in der überwiegenden Mehrzahl der Fälle bei den von oben gekommenen Teilchen, d. i. bei negativem w, ein positives u vorfinden; kurz, es besteht eine Korrelation zwischen u und w, derart, dass der Mittelwert $\overline{u \cdot w}$, für eine längere Zeit berechnet, wahrscheinlich negativ ausfallen muss. Angenommen nun, es handle sich um Molekeln, so ist w proportional der mittleren Geschwindigkeit, etwa c; bezeichnet man die »freie Weglänge« mit l, so gelangt eine Molekel, die sich diese Strecke auf oder ab bewegt, in eine Schicht, deren mittlere Geschwindigkeit um $l \cdot \frac{dU}{dz}$ von derjenigen der Ausgangsschicht verschieden ist, und für u wäre $l \cdot \frac{dU}{dz}$ zu setzen. Man bekommt daher für die ausgetauschte Bewegungsgrösse, die als »Schubspannung« an der Schichtgrenze auftritt (»Reynolds stress«), den Ausdruck $\tau = \varrho \overline{uw}$ $= \text{const.}\ \varrho c l \cdot \frac{dU}{dz} = \mu \cdot \frac{dU}{dz}$, und der molekulare Reibungskoeffizient wird $\mu = \text{const.}\ \varrho c l$.

Anders bei molaren Vorgängen. Hier tritt schon, bevor der Flüssigkeitsballen seinen höchsten oder tiefsten Punkt erreicht, ein Austausch der Bewegungsgrösse ein infolge der unausgesetzten Berührung mit der übrigen Flüssigkeit. Trotzdem kann man nach L. Prandtl den Ausdruck $\overline{u \cdot w}$ vereinfachen, wenn man zwei Annahmen macht:

(1) Auch jetzt noch sei die wagerechte Komponente u der Bewegungsunruhe, wenigstens der Grössenordnung nach, bis auf einen Faktor $= l \frac{dU}{dz}$, wo nunmehr l der »Mischungsweg« heisst.

(2) Die senkrechte Komponente entstehe in der Hauptsache dadurch, dass zwei etwa von oben und unten in eine Schicht eingedrungene Massen gegen diese den Geschwindigkeits-Unterschied $+ l \frac{dU}{dz}$ und $- l \frac{dU}{dz}$ haben, sich also gegen einander mit der Geschwindigkeit $2 l \frac{dU}{dz}$ bewegen und dadurch das zwischen ihnen befindliche Wasser

verdrängen, so dass es nach oben oder unten ausweicht; die senkrechte Komponente wird dann ebenfalls proportional zu $l \cdot \frac{dU}{dz}$ sein.

Auf Grund dieser beiden Annahmen würde daher die ausgetauschte Bewegungsgrösse, die als »Schubspannung« τ an der wagerechten Schichtfläche merkbar wird,

$$\tau = \pm \varrho \cdot \left(l \cdot \frac{dU}{dz} \right)^2, \tag{3}$$

oder, wenn man in Anlehnung an die Formel für molekulare Reibung schreibt,

$$\tau = A \cdot \frac{dU}{dz}, \tag{3a}$$

und der Austauschkoeffizient wäre

$$A = \pm \varrho l^2 \frac{dU}{dz}, \tag{4}$$

wobei das Vorzeichen stets so zu wählen ist, dass der Ausdruck A positiv wird, wie er übrigens in dem gewählten Beispiele von selbst der Fall ist. Prandtl zeigt, dass die für die Strömung »charakteristische Länge« l sich auch deuten lässt als Grössenmass für die auf und ab steigenden Wirbel. Eine noch andere Auffassung hat neuerdings Th. v. Kármán (1930) entwickelt; indem er annimmt, dass die Strömungszustände, auch die Schwankungen u, w, in verschiedenen Abständen z vom Boden einander ähnlich seien und sich nur im Grössenmasstabe unterscheiden, gelangt er für die übertragene Bewegungsgrösse oder Schubspannung zu dem gleichen Ausdrucke wie Prandtl, wobei aber l jetzt eine »für den Längenmasstab der Störungen charakteristische Länge« ist, welche sich durch den Ausdruck

$$l = \frac{dU}{dz} : \frac{d^2U}{dz^2} \tag{5}$$

darstellen lässt.

Man wird sich hüten müssen, den Begriff des »Mischungswegs« allzu mechanisch mit der »freien Weglänge« der Molekulartheorie zu identifizieren. Unter diesem Vorbehalte wolle man die Fig. 1 betrachten, die nach Versuchen von J. Nikuradse (1929) an einem gedeckten Kanale mit parallelen Wänden (Breite $2b$) gezeichnet ist. Sein Querschnitt war ein schmales und sehr langgestrecktes Rechteck, so dass in einer zu den kurzen Seiten parallelen Mittellinie keine Störungen von den entfernten Seitenwänden zu befürchten waren, und dass die eine Hälfte der Strömung an dieser Stelle als Modell einer seitlich unbegrenzten Strömung über ebenem Boden gelten kann. Es zeigt sich zunächst, dass die Geschwindigkeit im grössten Teile des Kanals nicht eben sehr verschieden gross und über $^4/_5$ der Kanalbreite durch eine gewöhnliche Parabel mit grosser Genauigkeit darstellbar ist, dass sie aber in der Nähe der Wand plötzlich abfällt, und dass eine letzte Schicht an der Wand haftet; diese Grenzschicht ist es, in der nach Prandtl die Zähigkeit den Ausschlag gibt, und von der aus fortgesetzt Wirbel in die übrige, von der Zähigkeit fast unbeeinflusste Strömung eindringen. Bei flüchtiger Beobachtung, wenn die manchmal nur dünne Grenzschicht übersehen wird, kann leicht der Eindruck

entstehen, als ob die Hauptmasse an der Wand entlang glitte, ohne zu haften. Die Schubspannung τ dagegen (die hier in willkürlichen Einheiten angegeben ist) nimmt gleichmässig von der Wand bis zur Mitte ab. Leitet man aus $\tau = A \cdot \frac{dU}{dz}$ die Austauschzahl A ab, so erweist sie sich als klein in Wandnähe; hier gibt also die schnelle Änderung der Geschwindigkeit $\frac{dU}{dz}$ den Ausschlag, so dass trotzdem τ gross ist. In der Kanalmitte aber verschwindet τ, weil sowohl A, wie $\frac{dU}{dz}$ Null werden, und dazwischen hat folglich

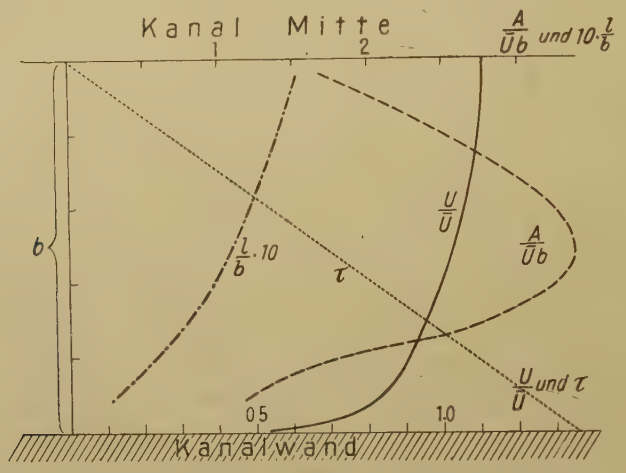

Fig. 1. Strömung in einem Kanale, nach J. Nikuradse.

——— Geschwindigkeit $\frac{U}{\overline{U}}$. ..... Relative tangentiale Spannung,

– – – – Austauschgrösse $\frac{A}{\overline{U}b}$. —·—·— »Mischungsweg« $\frac{l}{b}$ in zehnfachem Masstabe.

Für $\frac{A}{\overline{U}b}$ und $10 \cdot \frac{l}{b}$ gilt die obere, für $\frac{U}{\overline{U}}$ und τ die untere Skala. Damit die Zahlen der Figur allgemein gelten, sind U, l, A nicht in absoluten Werten, sondern im Verhältnisse zur Maximalgeschwindigkeit $\overline{U}$, zur halben Kanalbreite b, und zu $\overline{U}b$ angegeben.

A ein Maximum. Erst der »Mischungsweg« l ist eine Grösse, die, wie man leicht versteht, an der Wand klein ist, aber mit wachsendem Abstande von der Wand fortgesetzt zunimmt, wenn sie auch keineswegs eine Konstante ist, wie die »freie Weglänge« der Molekeln; eine Behauptung, die freilich von v. Kármán bestritten wurde, der für seine »charakteristische Länge« vielmehr eine Kurve ähnlich der für die Austauschzahl A in der Fig. 1 berechnet hat.

Mag dies dahingestellt bleiben, so ist doch zu erwähnen, dass auch bezüglich der Zahl selbst eine Reihe abweichender Meinungen besteht. Zunächst hat W. Schmidt sie definiert ohne Rücksicht auf die senkrechten Komponenten der Störungsgeschwindigkeiten, und sein Ausdruck für A enthält nur die mittleren Weglängen, die im Falle der Fig. 1 einen der A-Kurve ähnlichen Verlauf ergeben würden. Handelt es sich hier nur um eine andere Deutung des A, die den Zahlenwert selbst unangetastet lässt, so wird

auch dieser ein anderer, wenn man G. I. Taylor (1914, 1922) oder J. P. Jacobsen (1930) folgt. Ersterer findet, von der Konstanz der Wirbelstärke ausgehend, dass $\frac{d\tau}{dz} = \varrho\,\overline{wl}\,\frac{d^2U}{dz^2}$ sein sollte (also nicht, wie aus dem obigen Ansatze hervorgehen würde, $\tau = A\,\frac{dU}{dz}$, was $\frac{d\tau}{dz} = d\left(A\,\frac{dU}{dz}\right) : dz$ nach sich zöge), wo $\overline{wl}$ der Mittelwert der Produkte wl ist; letzterer kommt zu fast dem gleichen Ergebnisse, indem er für die Weglängen l eine Verteilung nach der Gauss'schen Fehlerkurve annimmt. Im Falle der Kanalströmung freilich ist der Verlauf der für $\overline{wl}$ berechneten Kurve von dem der A-Kurve der Fig. 1 nur insofern etwas verschieden, als das Maximum etwas höher liegen würde; die Kurve selbst ist fortgelassen, um die Figur nicht zu überladen. — Für die Meereskunde war daneben in den letzten Jahren eine ernste Schwierigkeit entstanden, weil die Berechnung der A-Werte aus Beobachtungen meistens auf sehr viel grössere Zahlen führte, wenn man von den Geschwindigkeitsunterschieden, als wenn man von denen der Temperatur oder des Salzgehalts ausging, und es ist deshalb um so mehr zu begrüssen, dass Prof. Taylor soeben einen Ausweg aus dieser Verlegenheit gewiesen hat.

3. Das Eingreifen der Erdrotation. Es erwächst nun die Frage, wie weit es möglich ist, die im Laboratorium gewonnenen Ergebnisse auf das Meer anzuwenden, was um so mehr erwünscht wäre, als es der Meereskunde noch allzusehr an Beobachtungen in dieser Hinsicht gebricht. Eine solche Anwendung begegnet jedoch Schwierigkeiten, nicht nur und in erster Linie wegen der schon erwähnten Schichtung des Weltmeeres, sondern namentlich, weil im Ozean die seitliche Begrenzung fehlt, wodurch die Drehung der Erde von bestimmendem Einflusse wird. Während im Versuchskanale die Wasserteilchen sich zwar mit verschiedener Geschwindigkeit, aber doch durchschnittlich in gleicher Richtung bewegen, werden in einem breiten Gewässer die Richtungen gegeneinander gedreht infolge der *cum sole*, d. i. auf der Nordhalbkugel nach rechts von der Bewegungsrichtung gerichteten Ablenkungskraft, und dadurch werden wiederum die virtuellen Reibungskräfte geändert, u. s. w. Wie sehr der endliche Ausgleich der Druck-, der Reibungs- und der Ablenkungskraft und das dadurch entstehende Strombild, sich von dem in einem Kanale unterscheidet, das hat bekanntlich vor einer längeren Reihe von Jahren V. W. Ekman gezeigt, allerdings unter der vereinfachenden Annahme, dass die Austauschzahl A konstant sei. Die Spitzen der die Geschwindigkeit darstellenden Pfeile liegen dann auf einer räumlichen Spirale (Fig. 2). Der Einfluss der Bodenhindernisse ist nunmehr begrenzt und reicht nur bis zu einem gewissen Abstande vom Boden, der »unteren Reibungstiefe« ($= D''$); weiter oberhalb bewegt sich die Flüssigkeit so, als ob es keine Reibung gäbe, d. i. 90° *cum sole* vom Druckgradienten: »Gradientstrom«; das ganze Stromsystem heisst »Staustrom«. Dreht man alle Pfeile in ein und dieselbe Richtung (Fig. 3), so hebt sich eine Bodenzone mit schnell zunehmenden Geschwindigkeiten ab, die ziemlich plötzlich in den fast konstanten Gradientstrom übergeht; wenn daher umgekehrt eine Geschwindigkeitskurve wie in Fig. 3 beobachtet würde, so dürfte man daraus noch nicht schliessen, dass A in der Nähe des Bodens einen anderen Wert hätte als weiter oben. Da übrigens ja A konstant angenommen wurde, $\frac{dU}{dz}$ und $\frac{dV}{dz}$ nach

oben hin sich aber der Null nähern, so würde dies nach (4), S. 47, bedeuten, dass l mit dem Abstande vom Boden unbegrenzt wachsen würde. Wollte man dagegen die v. KÁRMÁN'sche Formel (5) auf diesen Fall übertragen, so würde l sich einer Konstanten nähern. Beide Auffassungen, nach denen die von den Bodenwiderständen veranlassten Mischbewegungen auch in den tiefsten Meeren bis an die Oberfläche reichen würden, finden indessen in der Natur an Sprungschichten eine Grenze.

Ganz anders in einem Kanale, in dem bei konstanter Austauschzahl das »Geschwindigkeitsprofil« eine Parabel (Fig. 3, gestrichelt) sein würde. Die Parabel ist denn

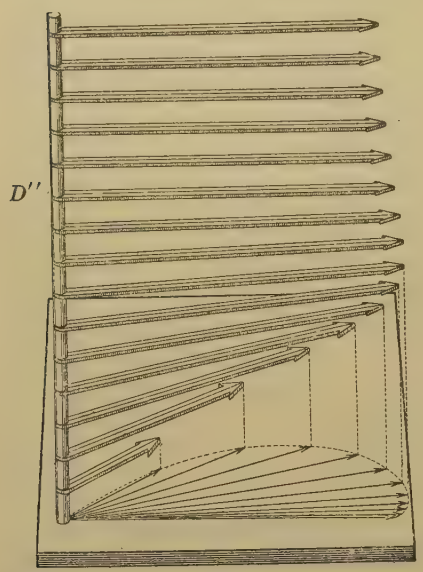

Fig. 2. Staustrom nach V. W. EKMAN.
D'' = Untere Reibungstiefe.

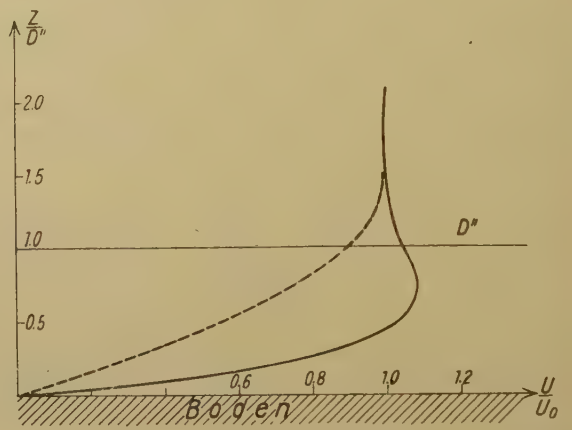

Fig. 3. Geschwindigkeitszunahme über dem Boden bei konstantem Reibungskoeffizienten
– – – – ohne } Einwirkung der Erdumdrehung
——— mit } (Theorie).

auch kürzlich noch von A. DEFANT (1930) für die kanalartigen Meerengen des Bosporus und der Dardanellen als zutreffend befunden worden, sofern man Gleiten am Boden annimmt; übrigens geht auch für breitere Gewässer die EKMAN'sche Kurve mehr und mehr in eine Parabel über, je kleiner die Meerestiefe gegenüber der Reibungstiefe ist, weil dann der Einfluss der Scheinreibung den der Erdumdrehung überwiegt. Wenn die DEFANT'schen Parabelprofile scheinbar den Ergebnissen von NIKURADSE (Fig. 1) widersprechen, so klärt sich dies wohl einfach dadurch auf, dass in den Meerengen keine Messungen im Bereich der Grenzschicht zur Verfügung standen; man mag die Gleitung am Boden, die sich in den DEFANT'schen Kurven findet, im oben dargelegten Sinne als Anzeichen einer Grenzschicht auffassen, und man könnte dann den wichtigen Schluss ziehen, dass die ungleich grössere Rauhigkeit des Bodens in den Meerengen die an einem glatten Kanale gefundene Geschwindigkeits-Verteilung nicht wesentlich änderte.

Unglücklicherweise besitzt man aus der Tiefsee so gut wie gar keine Beobachtungen, an denen man prüfen könnte, in welcher Weise sich die vereinfachende Annahme

eines konstanten Austauschkoeffizienten von der Wirklichkeit entfernt. Wohl dagegen sind in seichten Meeren einige, wenn auch nicht viele Beobachtungen gemacht worden. Aber hier tritt eine andere Schwierigkeit auf, indem Gezeitenströme die stationären Strömungen zu verdecken pflegen und natürlich auch für Mischung und Turbulenz den Ausschlag geben. Bei dem Versuche, die bisherige Theorie auf die periodisch veränderlichen Tideströme auszudehnen, geht man am besten davon aus, dass sich die einer Teiltide zugehörigen Ströme für jedes Niveau kartographisch durch eine Ellipse darstellen

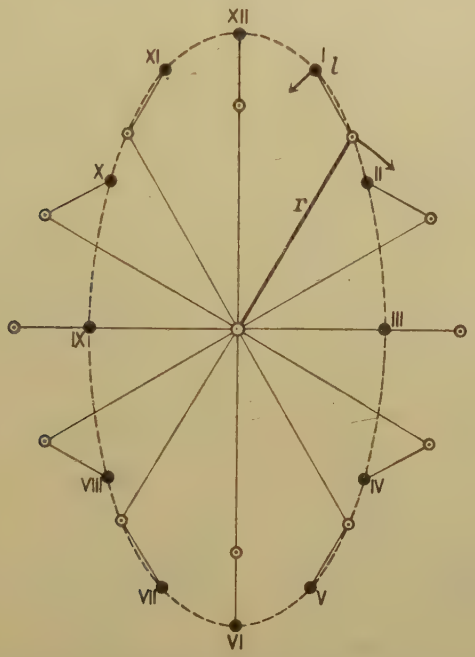

Fig. 4. Zerlegung einer elliptischen Bewegung in zwei entgegengesetzte Kreiskomponenten r und l. Die Pfeile geben den Umlaufssinn an.

lassen, welche von der Spitze des Geschwindigkeitspfeils, den man sich vom Mittelpunkte aus gezogen denkt, mit konstanter Flächengeschwindigkeit durchlaufen wird. Während man aber für gewöhnlich den Strom als Resultierende aus zwei zueinander rechtwinkligen Komponenten betrachtet, ist es hier zweckmässig, die elliptische Bewegung als Ergebnis zweier entgegengesetzter kreisförmiger aufzufassen (Thorade 1929), wie dies z. B. seit Fresnel in der Lehre von der Polarisation des Lichts üblich ist. Die Figur 4 zeigt, wie die Ellipse entsteht, wenn ein Punkt (durch einen offenen Kreis bezeichnet) nach Art eines Planeten um den Mittelpunkt kreist und seinerseits von einem Trabanten (dem schwarzen Punkte) umkreist wird. Beide Bewegungen müssen im Gegensatze zu einer wahren Planetenbewegung gleichförmig und einander entgegengesetzt sein. Nennt man die beiden Radien r und l, so ist die grosse Halbachse gleich der Summe $r + l$, die kleine gleich der Differenz der Radien, $r - l$. Ist $r > l$, wie in der Figur, so

dreht der resultierende Strom (römische Ziffern) rechts, im anderem Falle links herum, und wenn beide gleich sind, liegt alternierender Strom vor. Die Theorie lehrt nun, dass jede der beiden »Zirkularkomponenten« r und l, einzeln genommen, sich wie ein stationärer Strom verhält (Fig. 2 und 3): in irgend einem Zeitpunkte liegen sowohl die Spitzen der rechts wie der links drehenden Komponente auf einer räumlichen Spirale; aber die Abnahme zum Boden hin ist verschieden gross, indem die Reibungstiefe auf

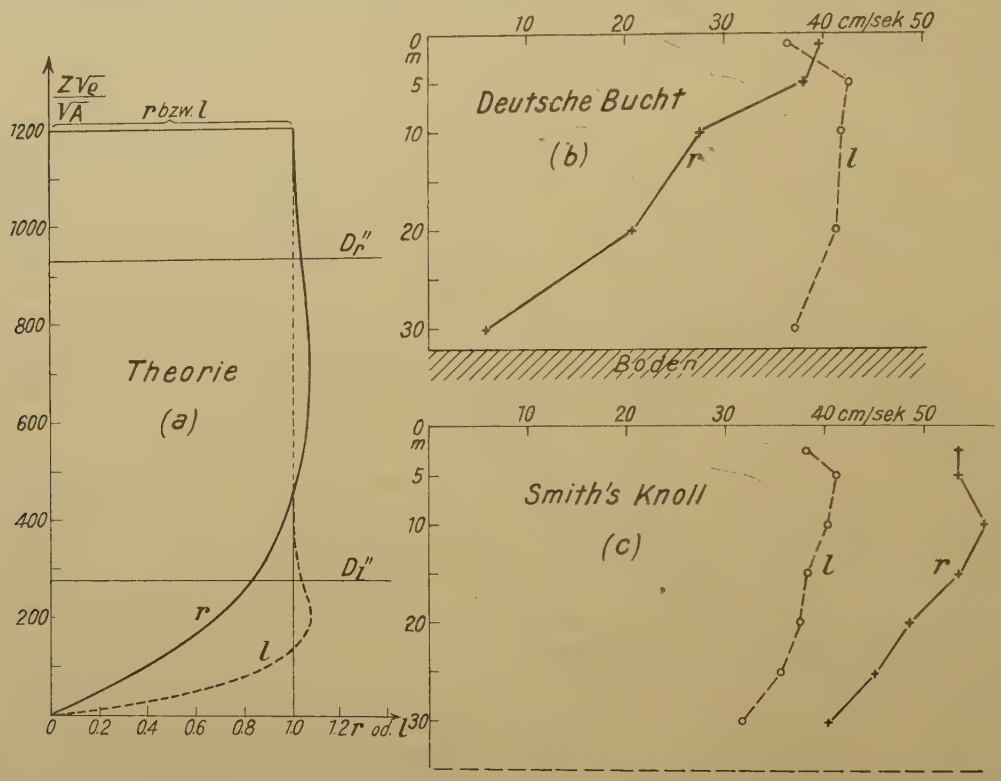

Fig. 5. Rechtsdrehende (———) und linksdrehende (– – –) Zirkularkomponente des Gezeitenstroms,
(a) nach der Theorie,
(b) nach Beobachtungen in der Deutschen Bucht,
(c) nach solchen bei Smith's Knoll (Wassertiefe etwa 35 m).

der Nordhalbkugel für r grösser ist als die für l, und zwar um so mehr, je näher dem Nordpole[1]). Wenn daher z. B., wie in Fig. 5a, r und l in grösserem Abstande vom Boden gleich gross sind (= alternierender Strom), so kommt in Bodennähe eine Zone, in der

[1]) In der geographischen Breite φ wird, wenn ω_0 die Winkelgeschwindigkeit der Erde, σ die der Gezeit, und $\omega = \omega_0 \sin \varphi$ ist, die Reibungstiefe für die rechtsdrehende Komponente $D_r'' = \pi \sqrt{\frac{2A}{\varrho(\sigma - 2\omega)}}$, für die linksdrehende $D_l'' = \pi \sqrt{\frac{2A}{\varrho(\sigma + 2\omega)}}$, während sie nach Ekman für stationäre Ströme $D'' = \pi \sqrt{\frac{A}{\varrho\omega}}$ wäre. Vgl. ausser der erwähnten Arbeit auch Fjeldstad (1929, S. 33 ff.).

mehr und mehr l überwiegt (links drehender Strom); wäre dagegen oben l etwas kleiner als r, so läge die gestrichelte Kurve oben links, unten rechts von der ausgezogenen und ihr Schnitt gäbe eine Tiefe an, in der sich der Drehsinn umkehrt. Wahrscheinlich ist es so zu erklären, dass Horns Riff in den oberen Schichten rechts, in den unteren linksdrehenden Strom hat. Die Fig. 5 lässt erkennen, dass (b) nach Beobachtungen der Deutschen Seewarte (Thorade 1928) in der Deutschen Bucht der Nordsee (auch hier eine Umkehrung des Drehsinns dicht unter der Oberfläche!) und auch (c) bei Smith's Knoll nach einer auf einer Arbeit von S. F. Grace (1929) fussenden Berechnung

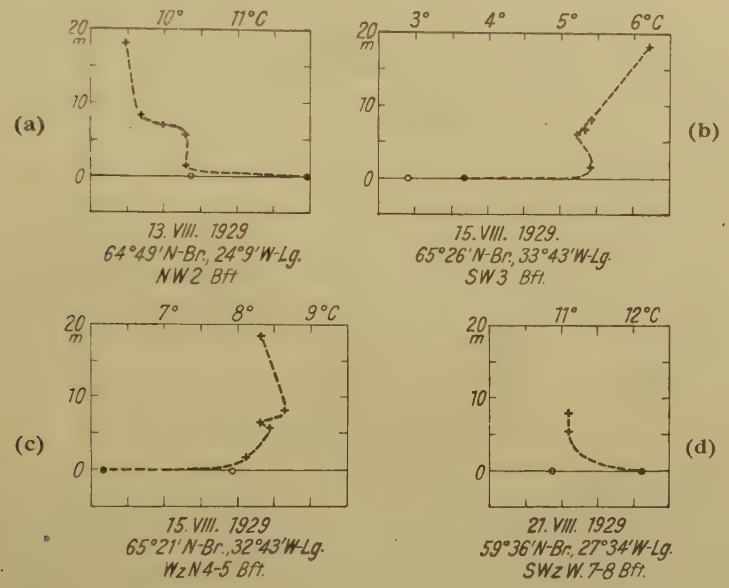

Fig. 6. Lufttemperaturen über dem Meere.
(a) Wasser wärmer als Luft, leichter Wind, (b) Wasser kälter als Luft, leichter Wind,
(c) frischer Wind, (d) stürmischer Wind.
Der dicke Punkt gibt die Oberflächentemperatur des Wassers an.

die verschieden starke Annahme der beiden Kreiskomponenten grundsätzlich mit den Forderungen der Theorie übereinstimmt.

Allerdings nicht dem Zahlenwerte nach! Vielmehr lehren (b) und (c), dass in unmittelbarer Nachbarschaft des Meeresbodens eine unerwartet schnelle Abnahme der Geschwindigkeit eintritt. Damit erweist sich die Theorie zwar in den Grundzügen als richtig, sie bedarf aber, besonders in Bodennähe, der Verbesserung. In die gleiche Richtung weisen auch Beobachtungen von Luftströmungen, die man hier zum Ersatz für fehlende Strombeobachtungen heranziehen kann. Es ist daher notwendig, die Annahme eines konstanten Austauschkoeffizienten zu verlassen und nach einer anderen Grundlage Ausschau zu halten.

4. Die Abnahme des Austausches in Bodennähe. Es ist nicht wohl zu

bezweifeln, dass die Mischungsintensität in den untersten Schichten abnehmen muss, weil hier die unregelmässigen Auf- und Abbewegungen durch die feste Begrenzung behindert werden. Dass dies in der Tat in hohem Grade zutrifft, diese Überzeugung drängte sich dem Schreiber dieses besonders deutlich auf durch eine Reihe von Beobachtungen der Lufttemperatur über dem Meere, von denen Fig. 6 einige Proben gibt. Nimmt man an, dass die Temperatur der untersten, am Wasser haftenden Luftschicht mit der Wassertemperatur übereinstimmt, so sieht man, dass der Temperaturgradient auch bei frischem Winde die Tendenz hat, sich auf die unterste, etwa $1^1/_2$ m dicke Schicht zu konzentrieren, sei es, dass die Luft — der häufigere Fall — kälter ist als das Wasser, oder dass sie wärmer ist. Man wird unwillkürlich an L. Prandtls schon erwähnte Grenzschicht-Theorie denken, nach der eine dünne Flüssigkeitsschicht an der Wandung haftet und hauptsächlich unter der Einwirkung der Zähigkeit steht, für die übrige Flüssigkeit aber nur als Ursprungsstätte der Wirbel wichtig ist, die den Austausch der Bewegungsgrösse besorgen. Freilich ist die $1^1/_2$ m-Schicht viel zu dick, um ohne weiteres mit jener Grenzschicht für identisch gelten zu können; aber auch die Rauhigkeit eines wogenden Meeres ist von einer anderen Grössenordnung, als die von Rohren und Versuchskanälen, und so ist es notwendig, die wenigen Beobachtungen am Luft- und Weltmeere selbst zu Rate zu ziehen. Drei Versuche in dieser Richtung seien hier als bemerkenswert aufgeführt:

(a) L. Prandtl und W. Tollmien (1925) gingen aus von den Ergebnissen, die an Versuchskanälen erhalten wurden, und die auf ein »Potenzgesetz« für die Geschwindigkeit von der Form

$$U = az^{\alpha} \tag{6}$$

führten, wo a eine Konstante ist; über den Exponenten α besteht keine volle Einigkeit; legt man ein von Blasius gefundenes Gesetz für die Wandreibung zu Grunde und nimmt an, dass diese nur von den Vorgängen in der Nähe der Wand, nicht aber von den Abmessungen des Kanals abhängt, so kann man nach Prandtl schliessen, dass $\alpha = {}^1/_7$ sein muss; die Versuche, auf die sich Prandtl und Tollmien stützen, führten auf ein etwas grösseres $\alpha = 0{,}157$. Lässt man ein Potenzgesetz, wie Formel (6), gelten, so findet man durch Einsetzen in (3a), dass die Austauschzahl

$$A = \text{const. } z^{1-\alpha} \tag{7}$$

wird, und daraus ergibt sich nach (4) leicht, dass auch der Mischungsweg l dem gleichen Gesetze gehorcht:

$$l = \text{const. } z^{1-\alpha}. \tag{8}$$

Beide Grössen, A und l, würden also etwas langsamer zunehmen, als wenn sie zum Abstande vom Boden proportional wären. Indem Prandtl und Tollmien diesen ursprünglich für Kanäle geltenden Ansatz auf die seitlich unbegrenzten Luftströmungen übertragen, in denen neben der Komponente U auch die dazu quer gerichtete V (wegen der Erdumdrehung) eine Rolle spielt, machen sie also die Voraussetzung, dass auch für V dieselbe Austauschzahl gilt, wie für U. Die etwas mühsame Rechnung führt im Gegen-

satze zu der Spirale der Fig. 3 auf die etwas flachere Kurve der Fig. 7 und kommt der Wirklichkeit zweifellos insofern näher als jene, weil der Wind in Bodennähe in der Tat mehr als 45° vom Gradienten abgelenkt ist; auch nimmt nach der PRANDTL-TOLLMIEN-schen Theorie die Geschwindigkeit in den unteren Schichten ohne wesentliche Änderung der Richtung ziemlich schnell zu, was ebenfalls zu den Beobachtungen stimmt. Zieht man aber z. B. die Windmessungen von G. M. DOBSON (1914) heran, so erweist sich der theoretische Ablenkungswinkel mit nunmehr 77,5° als zu gross.

Danach scheint es, als ob man die PRANDTL-TOLLMIEN'sche Theorie nach der Richtung weiter ausbauen müsste, dass man sie mit anderen Werten für α durchrechnete; würde man $\alpha = \frac{1}{n}$ wählen, wo n eine ganze Zahl ist, so würde man, worauf schon H. SOLBERG (1924) hinwies, die Lösung in Besselschen Funktionen von der Ordnung $\frac{1}{(n+1)}$ bekommen. Neuere Untersuchungen von J. NIKURADSE (1930) zeigen, dass mit steigender

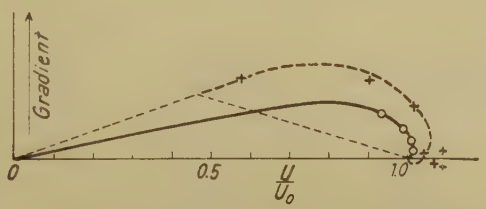

Fig. 7. Wind über ebenem Boden (U_0 Gradientwind, U Komponente quer zum Gradienten).
+ + + Beobachtungen nach DOBSON,
——— Theorie nach PRANDTL-TOLLMIEN,
-------- Theorie nach V. W. EKMAN — G. I. TAYLOR.

REYNOLDS'scher Zahl R der Wert von α abnimmt (bis = 0,101 für R = 3 Millionen); andrerseits jedoch fand W. SCHMIDT (1929) aus seinen unten erwähnten Windmessungen weit grössere Werte, von α = 0,19 bis 0,42 hinauf, was aber zum Teil vielleicht auf die Vernachlässigung der senkrechten Komponente der Luftbewegung zurückzuführen ist. TH. V. KÁRMÁN endlich hat (a. a. O.) ein Potenzgesetz überhaupt abgelehnt und es durch eine logarithmische Formel ersetzt, die zwar die bisher vorgeschlagenen Potenzgesetze in sich zusammenzufassen scheint, sich aber auf den hier vorliegenden Fall nicht ohne weiteres anwenden lässt.

(b) Hat es hiernach den Anschein, dass zwar ein Weg beschritten, aber das Ziel noch nicht erreicht ist, so liegt doch in einem Falle bereits ein Ergebnis vor: Die Durchrechnung einer von H. U. SVERDRUP auf dem Nordsibirischen Schelfe beobachteten Triftströmung, bei der der Austauschkoeffizient vom Boden bis zur Oberfläche stark zunahm, führte J. E. FJELDSTAD (1929) zu einer der PRANDTL-TOLLMIEN'schen ähnlichen Formel, jedoch mit einem Exponenten $\alpha = {}^1/_4$ (Fig. 8a); die zugehörige Geschwindigkeitskurve (Fig. 8b) passt sich in der Tat den Beobachtungen bedeutend besser an, als die unter der Annahme einer konstanten Austauschzahl berechnete (in der Fig. gestrichelt), und es ist wohl kaum ein Zweifel, dass hier Beobachtung und Theorie gut übereinstimmen. Ob man aber dies Gesetz auf Gewässer von beliebiger Tiefe übertragen

darf, ist noch nicht sicher; schätzt doch SVERDRUP die Reibungstiefe auf 55 m, d. i. auf das $2^1/_2$ fache der Wassertiefe (22 m), und es ist zu vermuten, dass die vom Boden herrührende Turbulenz sich hier durch die von der Oberfläche kommende verstärkt; ausserdem ist die Intensität der Mischung wahrscheinlich noch dadurch über das dem Triftstrome entsprechende Mass hinaus vergrössert, dass zu diesem ein etwa doppelt so starker Gezeitenstrom hinzutritt. Immerhin ist es von Bedeutung, dass sich hier ein so ungewöhnlich grosser Exponent wie $\alpha = 1/4$ ergibt.

(c) Einen anderen Ausweg gab schon 1906 V. W. EKMAN an angesichts der starken Abweichung des für den Bodenwind berechneten Ablenkungswinkels (45°) von dem beobachteten. Er schlug vor, für die untersten Luftschichten ein Hinweggleiten über den Boden in die hydrodynamischen Gleichungen einzusetzen, und unabhängig von ihm

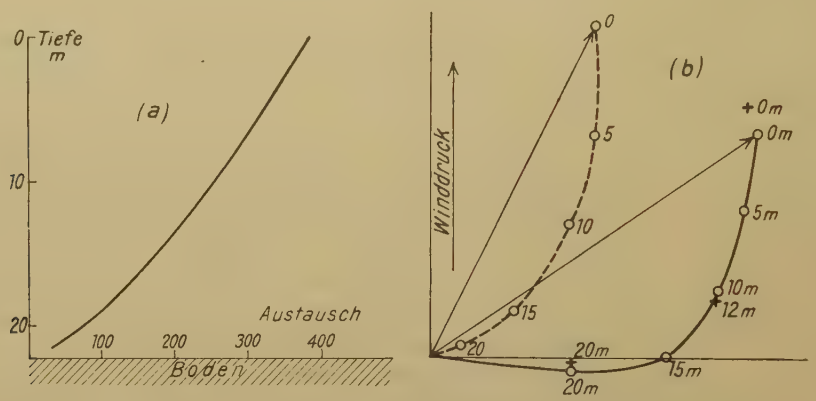

Fig. 8. Austauschzahl (*a*) in cgs-Einheiten und Stromdiagramm (*b*).
+ + + Beobachtungen von H. U. SVERDRUP,
——o——o—— Theoretischer Strom bei konstanter Austauschzahl,
——o——o—— Theoretischer Strom nach J. E. FJELDSTAD.

fand G. I. TAYLOR (1914) auf Grund der gleichen Voraussetzung und der Annahme einer konstanten Austauschzahl eine auch in Einzelheiten befriedigende Übereinstimmung der Theorie mit den Windmessungen DOBSONS (Fig. 7, gestrichelt). Dann würde nach der Formel (4) der Mischungsweg l mit der Höhe schnell wachsen, während er, wenn Formel (5) zutrifft, innerhalb des Gradientwindes konstant sein würde. Mit dem physikalischen Sinne des ersten geradlinigen Stücks der gestrichelten Kurve in Fig. 7, das sprunghaft in die Spirale übergeht, hat sich EKMAN in einer späteren Arbeit (1928) beschäftigt und die Gleitzone dahin gedeutet, dass, ganz ähnlich wie bei der PRANDTL'schen Grenzschicht, die untersten Luftteilchen am Boden haften, und dass dann eine Schicht folgt, in welcher der Austausch konstant, aber geringer als in den höheren Schichten ist. So behelfsmässig diese Zerlegung des Luftmeeres in zwei Teile mit unstetig zunehmender Austauschzahl A zunächst anmuten mag, so ist nicht zu verkennen, dass der Erfolg zu ihren Gunsten spricht; auch an die sprunghafte Änderung der Temperatur in der untersten Schicht (Fig. 6) mag man in diesem Zusammenhange denken; natürlich soll damit nicht eine Unstetigkeit im strengen mathematischen Sinne behauptet werden,

sondern nur eine so schnelle Änderung der Austauschzahl, dass sie in der Rechnung durch eine sprunghafte ersetzt werden darf. Was die Dicke dieser untersten Schicht angeht, so beschränkt sich Ekman in Ermangelung von eigentlichen Beobachtungen darauf, ihre Grössenordnung auf etwa $^1/_{100}$ der Reibungstiefe, d. i. für das Luftmeer, auf wenige Meter zu schätzen; im Ozean würde man dementsprechend wohl nach Dezimetern zu rechnen haben. Die Ekman'sche Grenzschicht, um sie kurz zu benennen, ist also der Grössenordnung nach wesentlich von der sehr dünnen Prandtls verschieden. Übrigens steht nichts im Wege, das Ekman'sche Verfahren der Teilung noch weiter zu treiben, und man kann dabei sogar, wie H. Solberg (1924) zeigte, die sprunghafte Änderung der Austauschzahl durch eine stetige ersetzen und damit die Geschwindigkeitskurve stückweise konstruieren. — Vielleicht wird im Meere der Austausch in der Bodenschicht noch mehr herabgesetzt, als in der Luft, da die untersten Wasserschichten häufig mit Schlamm oder Sand durchmischt sind, wodurch sich ihre Dichte vergrössert und eine stabile Lagerung hervorgerufen wird.

Zusammenfassend kann man über diese drei Theorien feststellen: Der aus Beobachtungen der Gezeitenströme und eines Triftstroms sowie der Luftbewegungen hervorgehenden starken Abnahme der Mischungs-Intensität in der Nähe des Bodens trägt Ekman Rechnung durch Absonderung einer Grenzschicht (»skin-layer«) und Einführung einer sich sprunghaft ändernden Austauschzahl; Prandtl-Tollmien und Fjeldstad legen eine passend gewählte stetige mathematische Funktion zu Grunde. Ob im Allgemeinen, abgesehen von den herangezogenen Beispielen, die Annahme einer stetig oder die einer unstetig (d. i. in gewisser Höhe sehr schnell) sich ändernden Austauschzahl den Tatsachen besser gerecht wird, dürfte nach den wenigen herangezogenen Beispielen nicht zu entscheiden sein, sondern bedarf intensiver weiterer Beobachtung.

5. Turbulenz im Einzelnen. So wenig ein Zweifel darüber bestehen kann, dass für die wichtige Frage der Mischung nur die statistischen Mittelwerte der Turbulenz in Betracht kommen, so ist es doch erwünscht, die Schwankungsbewegungen auch im Einzelnen kennen zu lernen, um daran die Berechtigung der Annahmen zu prüfen, die jenen Theorien zu Grunde liegen. Nur wenig ist bisher in dieser Hinsicht geschehen, worüber hier berichtet werden kann. W. Schmidt (1929) mass die Windverteilung über festem Erdboden und über einer Wasserfläche, indem er zwischen zwei um 10 m von einander entfernten Masten eine Reihe Drähte quer zum Winde ausspannte, an denen Pendel aufgehängt waren, durch deren Ablenkung die Windgeschwindigkeit gemessen wurde; die Pendelkörper bestanden aus Drahtringen mit darübergespanntem Gazenetz, so dass sie die Luftbewegung möglichst wenig verzerrten und infolge ihrer geringen Trägheit den Schwankungen schnell folgen konnten. Die Stellung der Pendel wurde kinematographisch in Zeitabständen von $^1/_7$ bis 1 Secunde photographiert und durch Diagramme mit Linien gleicher Geschwindigkeit ausgewertet; nach einem der Schmidtschen Augenblicksbilder ist die Fig. 9 als Augenblicksbild in Parallelperspektive gezeichnet worden. Schmidt kommt zu dem Ergebnisse, dass von wohlbegrenzten Wirbelkörpern keine Rede sein könne; der Austausch erfolge vielmehr durch geschossartiges Eindringen von Luftkörpern in andere Schichten, wie es auch die Figur 9 erkennen lässt, und auch dies geschehe nicht in breiter Front, sondern oft seien die Unterschiede der

Geschwindigkeit zwischen rechts und links grösser, als die zwischen oben und unten, ja gelegentlich sei unten der Wind sogar schwächer, als oben. Dass Wirbel gänzlich fehlen, während sie doch im Laboratorium bei Turbulenz regelmässig beobachtet werden, erklärt Schmidt damit, dass im Freien ein Wirbelfaden sich leicht von den Seiten her auffüllen könne, was dagegen im Versuchskanale wegen der Seitenwände nur sehr beschränkt möglich sei. Nun bedürfen allerdings diese Beobachtungen einer Vervollständigung, weil bei ihnen die senkrechte Windkomponente ausser Betracht bliebe; R. Becker (1930) hat hervorgehoben, dass etwaige Auf- und Abbewegungen der Luftmassen zwar auch die Pendel in Bewegung setzen müssten, aber nachher nicht mehr als solche erkenn-

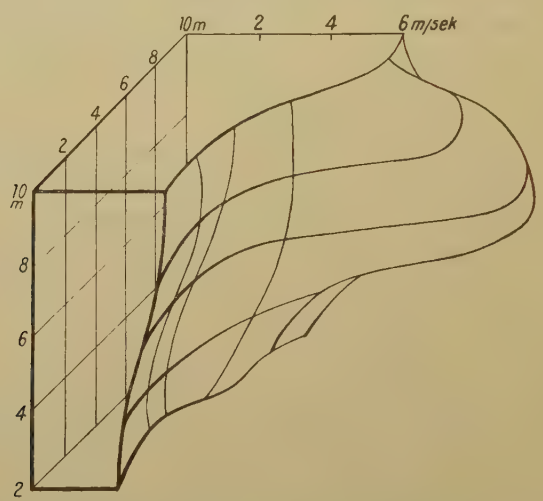

Fig. 9. Windgeschwindigkeit über dem Flugfelde Aspern bei Wien nach W. Schmidt (ohne Rücksicht auf die senkrechte Komponente).

bar wären, und dass daher die ausgerechneten Werte der Horizontalgeschwindigkeit durch senkrechte Bewegungen entstellt wären. In der Tat ist anzunehmen, dass sich das Bild bei Berücksichtigung der senkrechten Komponenten ändern wird, namentlich, was die quantitative Seite angeht. Ob aber die oben erwähnten Folgerungen allgemeiner Art entkräftet werden, steht doch noch dahin; denn sowohl das Fehlen der Wirbel als auch die bedeutenden Unterschiede zwischen rechts und links kehren in den Bildern W. Schmidts zu regelmässig wieder, um nicht mehr als Zufall zu sein.

R. Becker hat jedoch ausserdem die Forderung aufgestellt, man müsse nicht die Strömungen in einer Querschnittsfläche, sondern innerhalb eines ganzen prismatischen Raums untersuchen, um ein wahres Bild der Luftbewegungen zu erhalten; er hat deshalb über einer Fläche von etwa 20×20 m² zwei oder mehrere Stockwerke in 1,5 m bis 3,5 m Höhe über dem Erdboden mit Stromfahnen besetzt und, wie schon Schmidt es tat, die Windverteilung durch photographische Aufnahmen festgehalten. Dabei ergaben sich erstaunlich grosse Steigungs- und Gefällswinkel, Fig. 10 und 11, die sogar in einer Höhe von nur 1,5 m über dem Erdboden noch 40° überstiegen. Leider gestattete die Methode

keine Messung der Geschwindigkeit, und die Beobachtungen lagen nicht dicht genug, um Stromlinien daraus abzuleiten.

Treten schon im Luftmeere methodische Schwierigkeiten auf, so steigen sie ganz ausserordentlich für die dem Auge entzogenen Bodenschichten des Wassers. Über Beob-

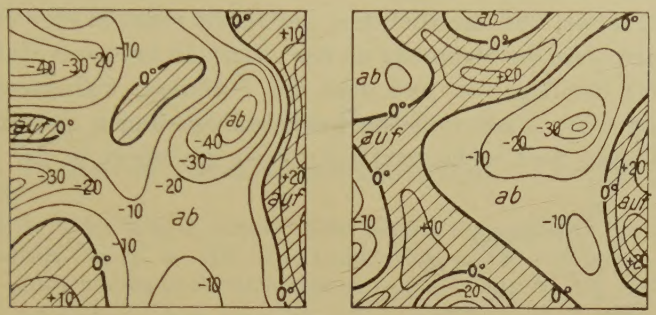

Fig. 10. Neigungswinkel der Luftströmungen: (a) in 2,8 m, (b) in 1,5 m Höhe über dem Erdboden. Das positive Zeichen bedeutet Aufwärtsbewegung. Nach R. Becker.

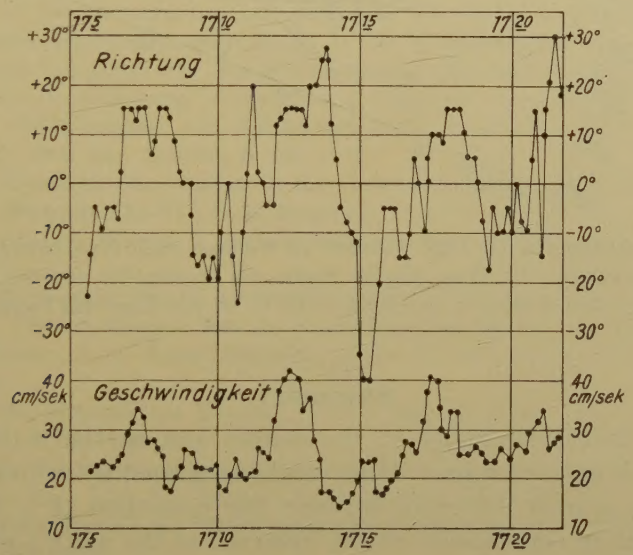

Fig. 11. Richtung und Geschwindigkeit des Stroms in der Elbe am 8. VI. 1926 nach H. Rauschelbach.

achtungen in Bodennähe ist dem Schreiber dieses zwar zur Zeit nichts bekannt. Einen gewissen Ersatz mag man jedoch erblicken in Beobachtungen, die H. Rauschelbach am 8. Juni 1926 mit dem von ihm gebauten Strommesser in der Elbe anstellte, und die er die Freundlichkeit hatte, zur Verfügung zu stellen. Das Instrument, das in 2 m Abstand von einem 80 cm tief gehenden Ponton aus 3 m unter der Oberfläche des 8 m tiefen Wassers ausgebracht war, registrierte in Zeitabständen von der Grössenordnung einer

Sechstel Minute und verzeichnete, wie die von RAUSCHELBACH entworfene Fig. 11 zeigt, ganz unregelmässige Schwankungen, sowohl der Richtung, wie der Stärke des Stroms, die etwa an Aufzeichnungen eines Böenmessers gemahnen können. Fig. 12 fasst beide Arten der Schwankungen in Vektorform zusammen, wobei allerdings die Zeit nicht mit wiedergegeben werden konnte. Innerhalb von 10 Minuten schwankt die Geschwindigkeit zwischen 14 und 42 m/sec, und die Richtung bestreicht einen Sektor von fast 70°. Allerdings ist anzunehmen, dass die Strömung an der unterhalb grösserer Hafenanlagen gelegenen Stelle besonders unruhig war, und dass die Turbulenz keineswegs immer so hohe relative Beträge erreicht; sonst müssten bei Strommessungen mit dem EKMAN'schen Instrumente die Kugeln weit mehr zerstrent liegen, als sie es in der Regel tun.

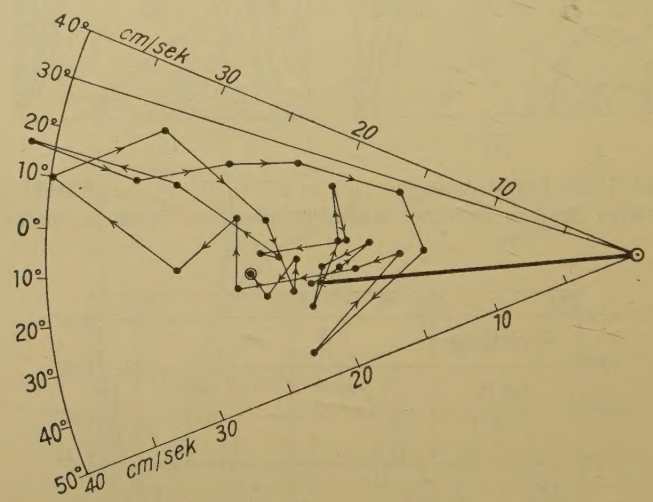

Fig. 12. Stromschwankungen in der Elbe während 10 Minuten nach RAUSCHELBACH. Die gebrochene Linie bezeichnet (schematisch) den Weg, den die Spitze des Strompfeils von der fett gezeichneten Anfangslage an bis zum Punkte ⊙ beschreibt. Mit 0° ist die Lage des Pontons bezeichnet.

Summary.

Turbulence arises principally on the surface and on the bottom of the sea; it is the latter which is concerned here. Much might be gained by utilization of laboratory work, but there is a great difference between the conditions of a laboratory-channel and those of the sea in consequence of the influence of the earth's rotation, as EKMAN has pointed out. The graph of velocity of a steady stream which (on the supposition of constant eddy-viscosity) is a parabola in a channel, a three-dimensional spiral in the sea, and it can be shown that a similar law governs the tidal streams. The increase of the velocity next to the bottom is a more gradual one in a channel than in the sea. In reality, the velocity increases still more suddenly than according to EKMAN's earlier theory. Therefore, (1) PRANDTL and TOLLMIEN, (2) FJELDSTAD and (3) EKMAN have suggested other laws of vertical distribution of velocity:— (1) the seventh root, (2) the fourth root of the distance from the bottom or surface, (3) a skin-layer adherent to the

bottom of the sea. The consequence is, in each case, a different intensity of mixing, i. e. "Austausch", which agrees more or less with observations. Recently, W. Schmidt and R. Becker stated a very complicated movement in the lower strata of the atmosphere, and temperature records obtained by the author showed a steep gradient in the lowest meter of the air; H. Rauschelbach, with his current-meter, found interesting details of turbulence in the River Elbe.

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Prof. R. Witting as acting Chairman thanked all the lecturers and expressed the hope that Prof. V. W. Ekman, Prof. Taylor and Dr. H. Thorade would take part in the discussions at the Scientific Meeting next year.

The discussion had to be given up for want of time.

Discussion of the Programme of the next Scientific Meeting.

Prof. **Knudsen** proposed the same programme for the Scientific Meeting next year:— Internal Waves and Mixing.

Prof. **O. Pettersson:**— In connection with what has been brought forward by Prof. Ekman it would be best to have a further discussion on internal waves next year.

Prof. **D'Arcy Thompson** asked if Prof. Witting could possibly give a survey of the sea-level investigations.

Prof. **Witting** thought it would be best to postpone the question about sea-level till 1933.

Prof. **Knudsen** asked if they were agreed regarding a programme on internal waves and mixing for the next year. This programme was adopted.